

The Determination of the Position of
a Momentary Impression in the
Temporal Course of a Moving Visual Impression

BY

N. TRIGANT BURROW

A Dissertation Submitted to the Board of University Studies of the Johns
Hopkins University in Conformity with the Requirements
for the Degree of Doctor of Philosophy

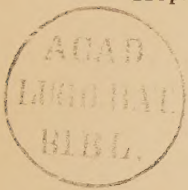
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THE DETERMINATION OF THE POSITION OF A MOMENTARY IMPRESSION IN THE TEMPORAL COURSE OF A MOVING VISUAL IMPRESSION.

I. Historical Survey.

Herbart originated the use of the word 'Complication' to designate the conjunction in consciousness of dissimilar impressions. Wundt, following Herbart's usage, applied the term 'Complication Experiment' to the specific investigation of the perceived temporal relation of a single impression of one sense to a continuous series of impressions of another sense, into the course of which it is suddenly introduced.¹ For the continuous series of impressions Wundt employed visual stimuli, and for the disparate impression an auditory one. The general conditions of Wundt's experiment present a situation analogous to that obtaining in the so-called 'eye and ear' method of the astronomers, to which it owes its inception; and his research presents, under conditions which subject it to accurate psychological measurements, a problem similar to that with which the astronomers were early confronted.²

In the use of the transit instrument, the field of view is divided vertically by a series of equidistant threads, the central one of which represents the meridian. The astronomer, who wishes to note the moment of a star's transit, *i. e.*, passage across the meridian, observes the time given by a clock beating seconds. Then, his eye at the telescope, he counts the beats of the pendulum, to determine the second at which the star coincides with the meridian line. Since it seldom happens that the star is coincident with the line at the precise moment of the pendulum stroke, the additional fraction of a second

¹ WUNDT, *Grundzüge der Physiol. Psychol.*, 4th ed., ii, pp. 401-; 5th ed., pp. 67-86.

² For a historical account of the "personal equation," see SANFORD, *Amer. Journal of Psychol.*, vol. ii, pp. 3-38, 271-298, 403-430.

is estimated by noting the position of the star at the stroke just preceding and at the stroke just following its passage across the meridian-line, and "dividing the time as the meridian-line seems to divide the space." Notwithstanding that a rigid control attempted to eliminate errors from every possible source, discrepancies invariably occurred in the computations of the time at which the star crossed the meridian, owing to the personal difference between various observers, the error frequently introducing a variation of more than a second in the readings. Bessel, who first noted this factor, gave to it the name of 'personal equation.' The analogy of the eye and ear method to the complication experiment will appear from a consideration of Wundt's procedure.

The apparatus which Wundt first had constructed for the purposes of his experiments, he called the 'complication-clock.'¹ It consists of a graduated circular dial, placed vertically and traversed clock-wise by an index-hand. The mechanism is that of a large clock, run by a weight, and having a fan-regulator for maintaining constant velocities, which however, may be arbitrarily varied within limits. It is so constructed that a bell-stroke may be produced coincidently with the alignment of the index-hand with any chosen division of the scale on the dial, the point selected on the scale being unknown to the observer. The index-hand of Wundt's apparatus was 25 cm. in length, equal to the radius of the dial measured to the proximal end of scale divisions; *i. e.*, the index-hand extended outward only as far as the scale divisions, so that in rotating it did not pass over them but moved by their inner ends.

Comparing now the elements in the mechanism of the complication apparatus with those of the eye and ear method of the astronomers, it is seen that the equidistant threads dividing the field of view of the transit instrument correspond to the series of scale divisions on the complication dial, that the star is represented by the index-hand, while to the pendulum-stroke of the clock beating seconds corresponds the auditory

¹ For a complete description of Wundt's complication-clock, see GEIGER, *Philos. Stud.*, vol. xviii, p. 349.

impression entering regularly into the course of the series of visual impressions of Wundt's complication apparatus. From this comparison it is clear that the errors in the estimated time of coincidence of star and meridian are analogous to the time-displacements occurring in the attempt to judge the position on the scale at which the passage of the index-hand is synchronous with the bell-stroke, as presented in the complication experiment. Later, Wundt substituted for the complication clock, with its uniform rate of rotation, a form of apparatus with index-hand actuated by the swinging of a pendulum, with a view to introducing the factor of varying acceleration.

The second form of apparatus, as distinguished from the complication-clock, he called the complication-pendulum. As in the complication-clock, the position of the index-hand (with regard to the scale), to which the bell-stroke corresponds, may be varied at will; but there is a difference in the movement of the index-hand, in that, as it now depends upon the swinging of a pendulum, it no longer rotates as before, but oscillates back and forth across the upper half of the dial, the instrument being so contrived that the bell-stroke occurs only when the index-hand is moving in the direction corresponding to that of the hands of a clock; *i. e.*, from left to right. The pendulum is at the center of its swing when the index-hand is in alignment with the upper vertical radius of the dial, to the right and left of which, depending upon the direction of movement, it is accelerating or retarding, in accordance with the law of harmonic motion. The radius of the scale and of the index-hand, which traverses the inner margin of the scale as in the original form of apparatus, is 17 cm. The scale is a half-circumference in extent.¹

Wundt was himself 'subject' throughout his experiments. When using the clock-machine, he stood in front of the dial, which was concealed by a wooden screen until the index-hand had attained the desired speed. He adopted no rigid method of observation, but in a manner as free from prejudices as possible, awaited the moment at which, after a sufficient num-

¹ For full description of pendulum-apparatus see WUNDT, *Physiol. Psych.*, 5th ed., vol. iii, p. 80.

ber of revolutions, he felt secure in naming the position upon the scale at which the index-hand appeared to be at the stroke of the bell. He noted this point; then, stopping the machine, compared his judgment with the actual position of the bell-stroke, which was determined by slowly rotating the index-hand until the bell-stroke occurred. Then he fixed the position for the next experiment, making the change carelessly, so that he did not know the new position. The rates of rotation which Wundt used varied between one revolution in a little less than 2 secs. and one revolution in a little more than 8 secs. When the index-hand revolved at a rate much slower than 8 secs., its position was clearly visible; when much more rapid than 2 secs., it was not possible to obtain a judgment.

In the outcome of his enquiry, Wundt found that correct judgments were the rare exception; that is to say, that it was seldom, and then apparently by accident, that the subject's *feeling* of the simultaneity of the sound with a given position of the index-hand agreed with the *actual* simultaneity of the two impressions. The subject selected a position on the scale at which the index-hand arrived either after the sound occurred or before it occurred. That is, in the former case, the sound impression was as if too tardy, the index-hand having already passed the assigned position, whilst in the latter case, the sound impression was as if too premature, the index-hand not yet having had time to reach this position before the sound was perceived. These two classes of error Wundt called arbitrarily *positive* and *negative*; positive errors occurring when the assumed position of simultaneity is later than the real one, and negative errors when the assumed position is earlier than the real one. Wundt found, contrary to the natural expectation, that negative errors were largely preponderant, it being the prevailing tendency to anticipate the sound, as it were.

Among the factors modifying the results, Wundt regarded the influence of speed as of chief importance. This is manifested in the difference in the quality of the error, according as the rate of rotation is slow or rapid; the negative errors predominating with slow speeds, whilst with greater velocities the positive errors prevail. At a certain point between the ex-

tremes of speed the average error becomes zero. This indifference point Wundt found to lie within the range of rotation-rates from 5 to 2 secs. With the complication-pendulum, with which he operated in the subsequent experiments, Wundt obtained results which agreed in the main with the preceding. The especial feature which Wundt noted in the use of the pendulum apparatus was a characteristic tendency to more negative errors if the bell-stroke occurred during a phase of acceleration of the index-hand (as was the case when moving from the left upward toward the median-line of the dial) and the tendency to more positive errors when the bell-stroke occurred during a phase of retardation of the index-hand, (as when moving downward from the median-line to the right-hand limit of the scale).

W. von Tschisch, who succeeded Wundt in experiments upon the complication problem, also made use of the pendulum machine.¹ He too was 'subject' for his own experiments. His results, in the main, corroborated Wundt's, the negative errors becoming less negative with increasing speed; but his average absolute errors were invariably negative, when experimenting, as Wundt, with a single complicating stimulus. The rates of oscillation of the pendulum, and therefore of the index-hand, used by von Tschisch, were one oscillation in 2 secs., in 1.5 secs., and in 1 sec.

The chief interest of von Tschisch's investigations consists in the substitution of other complicating impressions for the auditory impression of Wundt, and in the combination with sound impressions of other stimuli and of these in varying ways among themselves. For example, employing a touch-impression instead of sound, the results showed no noticeable change. However, where one or more disparate impressions were simultaneously combined with sound, the negative error was made smaller, and the error shifted progressively in a positive direction in proportion to the number of disparate stimuli which were thus combined. When three disparate complicating stimuli were employed, the error was positive.

¹ Ueber die Zeitverhältniss der Apperception einfacher und zusammengesetzter Vorstellungen. *Philos. Studien*, ii, pp. 603-634.

The occurrence of two disparate impressions within the same sense realm, but differing in quality, as the stroke of a bell and that of a hammer, have the like effect of diminishing the negative error. Von Tschisch further observed that the diminishing effect upon the negative errors exerted through adding to the number of momentary simultaneous impressions, was far less under conditions which admitted of an association between the impressions in question; *e. g.*, stimuli upon closely adjacent areas of the skin, wherein the touch impressions are united in a single space concept. If instead, widely separated areas of the skin be stimulated, the subject reacts as if disparate impressions were employed.

The next investigator in the field of complication-work was C. D. Pflaum.¹ He too used the pendulum form of apparatus. He experimented on himself and on two other subjects. His results concur with those of the two former Leipsic experimenters, *i. e.*, they show a preponderance of negative errors, which with increasing speed manifest a positive trend. The rates of pendulum oscillation employed by Pflaum were 2, $1\frac{2}{3}$, $1\frac{1}{2}$, and 1 seconds per oscillation.

It may be said here that all of the above experimenters are in harmony in respect to the essential returns. All are in accord as to the importance of the factor of the rate of rotation in determining the quality of the errors, the tendency to negative errors predominating with the slow speeds; and the gradual increase of positive errors with increased velocities. The experiments of both Wundt and von Tschisch number in the thousands, those of Pflaum number several hundred.

We next come to consider the studies upon the complication phenomena which were carried on in the Harvard Laboratory by J. R. Angell and A. H. Pierce.² These investigators resorted again to the method of uniform velocities and for their purposes constructed an apparatus similar in principle to the complication clock of Wundt. An ordinary kymograph served

¹ Neue Untersuchungen über die Zeitverhältniss der Apperception einfacher Sinneseindrücke am Complicationspendel. *Philos. Studien* xv, pp. 139-148.

² Experimental Research upon the Phenomena of Attention. *Amer. J. of Psychol.*, iv, pp. 520-541.

as the motive power. The chief difference between it and the older instrument consisted in the substitution of a sharp click for the reverberating bell-stroke, and of a dial which was but a portion (about $\frac{1}{6}$) of a circumference. The arc of the circle represented by the dial was below the center, its middle point lying on the lower vertical radius of the circle, so that as the index-hand revolved in the direction of the hands of a clock, its direction, when crossing the dial, was from right to left, and the index-hand, when approaching the center of the dial, moved in a downward course, just the opposite of the course of the index-hand in the complication pendulum, in which the arc of the circle traversed was above.

The index-hand which Angell and Pierce employed was much longer than the index-hand of the Wundt machine, measuring 47 cm. from center to tip. The complicating stimulus consisted of a sharp click produced by an electric hammer. The circuit through which the hammer was actuated was closed by a wire fastened to the index-hand and dipping into a mercury cup behind the scale. The position of the mercury cup along the scale was variable, and hence the sound could be fixed for any desired position of the index on the scale. In method, Angell and Pierce departed from the hitherto customary plan of leaving the subject quite free and untrammelled as to his procedure and adopted a usage which was, in their own words: "to follow the pointer on its first revolution until the sound is heard, when we attempt to stop the movements of the eyes instantaneously. The point on the dial thus attained is made the basis of operation for the next revolution, when any seemingly needed correction is made." They chose three rates: a slow, a medium, and a rapid rate, representing one revolution in 6.06 secs., 3.6 secs., and 1.2 secs. respectively. Angell and Pierce had presumably but few observers, as their experiments amounted to a comparatively small number. Their results show a wide divergence from those of the preceding German experimenters. In the first place they find on the whole, in contradiction to Wundt and his followers, a preponderance of positive errors appearing in the results of each subject toward the end of their series of experiments, which

indicates in the light of their interpretation the presence of a hitherto unrecognized factor, namely, that of practice. In the second place their results were further at variance with those of the preceding investigators in that they discovered no influence of velocity. The factor of varying acceleration did not come into court, since they worked with uniform rates of rotation.

In a comparative survey of the results of the two sets of investigators and of the conclusions reached by them with regard to the basic factors operative in producing the errors in the complication experiment, it appears that the German experimenters find a preponderance of negative values, while contrariwise the Americans find that the tendency to positive errors is the prevailing one, at least after practice. Further, the German investigators find that the negative values predominate with the slower speeds and the positive with the faster. With them no influence of practice is manifest. Angell and Pierce on the other hand deny the influence of speed in determining the quality of the judgment, but emphasize the influence of practice, the effect of which is gradually to shift the errors from the negative direction toward the positive. So that the conclusions of Angell and Pierce, as to the result of their inquiry were: influence of practice and no influence of speed, and of the Leipsic experimenters: influence of speed and no influence of practice. As said above, Angell and Pierce used constant rates of rotation, so that the question of the influence of varying acceleration inseparable from experiments with the pendulum apparatus did not enter into their determinations.

Such was the conflict between the returns of the Leipsic and the Harvard investigators until M. Geiger, working in Wundt's laboratory, attacked afresh the problem of complication.¹ This investigator reverted to the original type of apparatus, represented by Wundt's complication clock, but he departed from Wundt's procedure, in abandoning the use of the wooden screen for concealing the apparatus in the intervals of experimentation, preferring simply to disconnect the sound-stimulus

¹ Neue Complicationsversuche. *Philos. Studien*, xviii, pp. 347-436.

instead, as the repeated raising and lowering of the screen tended, in the opinion of this experimenter, to confuse the subject and vitiate the results. He also substituted for the bell a sharp hammer stroke, as the reverberations of the bell-stroke failed to satisfy the requirements for a strictly momentary auditory impression, and so did not fulfill the conditions essential to the experiment.

Since in the beginning of his work Geiger's method was directed by the purpose of a series of experiments serving as a control upon the results hitherto attained, he at the outset sought to eliminate the coefficient of individual peculiarity and to this end enrolled seven observers for the conduct of his experiments, a larger number than had been employed by any previous experimenter. With a view to isolating the influences of practice and of speed as factors determining the quality and magnitude of the errors, Geiger began with each observer with a speed of one rotation in 8 secs., and with successive sittings progressively decreased the time of rotation by half-second decrements to 1 sec., and finally to 0.9 sec., the extreme of speed to which his apparatus was adjustable. He then reversed the proceeding, beginning with a rotation-rate of one revolution in 0.9 sec., increasing the time to one revolution in 1 sec. and from here upward by $\frac{1}{2}$ sec. increments till a rotation-time of 8 secs. was again reached.

Geiger, whose work, as indicated, is primarily a control upon the conflicting results of his predecessors in the field, reconciles, in the main, the contradictory aspects presented by the conclusions of Wundt and his pupils on the one hand and those of Angell and Pierce on the other. He shows that both factors are operative—the influence of speeds and the influence of practice.

Having proceeded as above described, Geiger constructed a curve that traces the effect of his first series of experiments, that namely in which he used gradually increasing rates of speed, to illustrate the joint influence of practice and of increasing velocity—the two determinants going hand in hand in such a series. The curve thus derived represents a gradual progression from a markedly negative beginning to a termination upon the positive side. Now if the conclusion of the Leipsic

experimenters is correct and the positive tendency in the above series is due to speed alone, then the tracing of a second curve drawn from comparison and derived from a precisely contrary proceeding, *i. e.*, setting out with the fastest rate and gradually passing over to the slowest, ought to proceed in a direction just the reverse of the former, and represent an approximate retracing of the first curve in the opposite direction. But if, on the other hand, the assumption of the American observers is correct and practice alone is the decisive factor, the continued practice in operation after reversing the direction of change of velocities ought, on the contrary, to be represented by a curve that mounts still higher in the positive direction, continuing the course of the original curve. Now the curve actually resulting in the case of each of Geiger's subjects is found to be consistent with neither assumption, but is clearly the resultant of both forces —namely the influence of speed and the influence of practice; for the second curve is neither a reversal nor a continuation of the first, but exhibits a trend which in the main fluctuates between the influences of the two factors, the tracing for some observers tending feebly toward the positive, in others as faintly toward the negative side. True, there is a slight partiality toward the latter course, since in both curves the influence of speed is uniformly operative throughout, while from first to last the influence of practice is a decreasing factor and with the commencement of the second curve its efficacy has considerably waned.

With one of Geiger's subjects the influence of speed is shown in isolation. Here the succession of speeds proceeds irregularly, yet the rise of the curve coincides with the increased velocities. Further the influence of practice is manifest in individual experiments in which it happens that speeds which give an error of four or five scale-divisions, later reduce the error to nil. According to Geiger, the influences exerted by practice upon the errors are chiefly two. 1. With constant velocities increasing practice makes the errors constantly less negative or more positive. 2. Practice causes the maximal negative error (or minimal positive error) to occur at a speed greater than that at which it previously occurred.

From the above experiments, it is evident that Geiger's results are in full accord neither with those of the Leipsic nor with those of the Harvard observers. Geiger seeks to explain the discrepancy between Wundt's results and those of Angell and Pierce and himself with regard to the influence of practice, on the ground of the difference in apparatus employed. The pendulum-apparatus which Wundt used precludes by reason of its constantly varying speeds the possibility of effects of practice at any speed, while, on the contrary, the rotation device of Angell and Pierce and the complication-clock of Geiger favor the introduction of such effects through the uniformity in the movement of the index-hand. The explanation of the divergence of Angell and Pierce's results from Wundt's and his own in their failure to detect any influence of speed may be more fittingly stated after the consideration of the other constituents which Geiger includes in the sum of factors entering in to determine the results.

As a factor influencing the magnitude and the direction of the errors, Geiger lays stress upon the difference in the manner of observing characteristic of different individuals and accordingly distinguishes two types of observer, which he calls, for convenience, '*naïve*' and the '*reflecting*,' and names the corresponding methods, the '*index-hand*' and the '*scale*' method of observation. Characteristic of the '*naïve*' or '*index-hand*' method is the passive, quiescent, leisurely attitude of the subject. Quite indifferently he permits the impression of the *region* of probable simultaneity of sound and index-hand to arise within him, and later, with successive rotations, he selects, within this region, or rather there selects itself, as it were, the position at which the index-hand and the stroke are synchronous. The observer of the '*reflecting*' type on the contrary is tense and alert. He is bent upon catching as quickly as possible the exact position of the index-hand at which the sound enters. He does not wait composedly until, after several rotations of the index-hand, the apparent region of simultaneity is established, but seeks to determine its position upon the scale at the first entrance of the sound. He now holds strongly to the scale-division thus selected and with

the next revolution fixes it by the reading on the scale and then tries further to confirm his choice with the succeeding revolutions, or if unsuccessful, to make the needed correction, depending upon whether the sound appears to come before or after the point selected. Thus it is seen that careful thought and more or less of computation enter into the determination of the resulting judgment and give the cue for designating it as of the 'reflecting' type.

The different attitudes of the two types of observers toward the judgment made upon the first revolution of the index-hand is significant. One of Geiger's subjects who was of the 'reflecting' type, records that if he succeeds in getting a judgment on the first rotation, he has a feeling of security as to his determination, but Geiger himself, whose method was 'naïve,' finds the first judgment (the judgment obtained upon the first rotation of the index-hand,) relatively of no importance for his subsequent determination.

It is noteworthy that the 'naïve' subjects were able to observe according to the 'reflecting' method when directed to make the attempt, while the reverse was not readily the case. A further point of differentiation between the opposed types is to be seen in the importance of the rôle which accidental fixations of the attention play in 'naïve' judgments, as evidenced by the change of reading with successive rotations—each such change ranging over an interval of as many as 3 or 4 scale-divisions.¹

Geiger's results show that for those of his observers whom, upon the basis of their method of procedure, he had classified as 'reflecting', the two curves above mentioned lie for the greater part upon the positive side, while those belonging to the 'naïve' type presented curves which lie upon the negative side. With the subjects in whom Geiger found neither type characteristically presented, but in whom the method consisted in a mingling of the two procedures, the curves follow courses which in general are intermediate to those of the distinctly discernible types. Since in the light of the method of experimentation employed by Angell and Pierce their procedure

¹The scale was divided into 100 parts

clearly corresponds to the 'reflecting' type described by Geiger, he refers the preponderance of positive results shown by them to their experimental usage and traces to this source the causes of the discrepancy between the returns of these experimenters and those of Wundt. However, in seeking to base an explanation of the negative errors of the Leipsic experimenters on a reverse procedure, the clue did not prove to be the correct one, for in cases in regard to which information was obtainable, Geiger learned that the procedure adopted by these investigators included both methods, so that he was obliged to seek for other explanations for the peculiar results of these experimenters, and finally ascribed the difference of result to the different apparatus used. For obviously, owing to the difficulties attendant upon the inconstancy of the velocity which is a condition of the pendulum apparatus, the factor of practice is eliminated, and the two methods of observation are no longer characteristically present, but (Geiger maintains) lose their specific qualities and merge more or less into one another. We shall return to the question of temperamental types in a further connection.

It has been shown that increasing speeds of rotation increase the positive tendency of the errors. But increase in the rate of movement of the index-hand goes hand in hand with increase in the rate at which the momentary or disparate impression occurs. Hence the question arises: To which of the two factors is the result attributable? In the 'eye and ear' method of the astronomers it has been observed in some cases that positive errors increase with the more rapid course of the star¹ and in this case of course the rapidity of the disparate impression (clock-tick) is unchanged. It had also been shown by Bessel that altering the rate of the clock beat alone might change the error in the observation of the star's transit. Geiger attempted to investigate the influence of altering the rate of the successive strokes of the sound alone, the visual impressions occurring at the same rate. This he did by taking a series of experiments in the usual way and alternating with a series in which the hammer-stroke occurred only at alternate

¹ Sanford, *Op. Cit.*, pp. 287-8.

revolutions of the index, so that the rate of auditory rhythm was decreased to just half the original rate. In his account of this variation of his experiments, Geiger is unsatisfactory. He says at one place that the effect of omitting the alternate strokes is a greater positive tendency, at another that it is a less. Further, his final statement is not in accord with his tables. This method of determining the influence of the slower auditory rhythm is inadequate at best, for in thus introducing two revolutions of the index-hand for each auditory stimulus, Geiger arbitrarily neglects the factor of the alternate blank rotation of the index-hand and of the possible effect of this upon the attentive process (in deflecting the judgment to one side or the other). Moreover there is no certainty that the actual rhythm is changed at all by this method, since the interval between two strokes at the slower rate may be broken up into two attention-periods, corresponding to the periods of the fast rate.

An interesting point first brought out by Geiger is the factor of positional error, dependent upon the position of the index-hand at the moment of the auditory stimulus, in one case on the right hand side of the scale, in which it has a downward direction, and in the other case on the left hand side, in which position the direction of its movement is upward. The experiments which Geiger undertook to test the influence of positional error show with remarkable regularity that the deviation of the average error upon the right side of the scale from the general average error is in the positive direction, and upon the left side in the negative direction, the maximal negative error in the latter case lying toward the beginning of the upward course of the index-hand, while the positive influence upon the errors tends to a more uniform distribution upon the corresponding side of the scale.

Von Tschisch earlier called attention to this difference in the tendency of the errors upon the opposite sides of the dial, but he attributed the source to the acceleration of the index-hand, characteristic of the pendulum-machine, when moving upward as on the left side, and to the retardation when moving downward as on the right. Apparently von Tschisch

was upon the wrong track, since Geiger observed the influence of the same factor with the uniform velocities of the complication-clock.

Wundt and Pflaum, who, as has been said, made use of the pendulum machine, also performed experiments which showed the stronger negative influence of the upward moving index-hand when the upward movement corresponded to the phase of acceleration, or the stronger positive influence of the downward movement, when this coincided with the phase of retardation of the index-hand. But these observers failed to discriminate severally the two factors of which their determinations expressed the resultant. The attempt at the differentiation of the factors here involved was undertaken by a later investigator who will be mentioned below. The problem with which Geiger was concerned had to do with the simpler conditions presented in the clock machine, in which the factor of change of acceleration was eliminated, allowing the isolation of the influence of the different directions of motion on the lateral halves of the dial.

In order to rule out a possible defect of the apparatus which might operate in such a way as to cause an actual difference in the movement of the index-hand upon one side or the other, Geiger obtained an inverted image of the dial by interposing a telescope between it and the observer, the observer's visual field being exactly coterminus with the dial. Thus the uppermost and lowermost points being interchanged, the index-hand now moved in a downward course toward the point which before was the top of the dial and in an upward course toward the point which before was the bottom. That is to say, the image of the hand moved down when the real hand moved up, and *vice versa*. Notwithstanding the relative reversal of the positions of the dial in regard to this upward and downward direction of movement of the index-hand, the results were unaltered—the errors being more negative on the side of apparent upward movement, or more positive on the side of apparent downward movement, thus clearly excluding the possibility of irregularity in the mechanism of rotation as a cause of the difference in errors upon the two sides of the scale.

Geiger subjected to a similar test the influence of a possible predilection of the subject for the right or left side in causing the difference in the quality of the judgments, by reversing the two sides. This he did by having the subject observe the image of the dial reflected in a mirror. Again the results were substantially unchanged, showing that the direction of rotation from left or right was inconsequential.

Finally, Geiger proceeded to test the influence of the peripheral factor of eye-movements occurring concomitantly with the upward and downward movement of the index-hand. To this end he experimented in a darkened room and by replacing the original form of index-hand and scale-divisions with slits in a black surface and made visible by light entering from behind, he was able to observe the relative breadth of the after-image obtained during movement of the index-hand in the two directions. The results showed an after-image of greater extent upon the side of upward than upon that of downward movement, thus introducing a condition favorable to more negative readings upon the side of the ascending direction of the index-hand, as compared with those of the descending.

While Geiger disentangled the elements of acceleration *versus* retardation upon the one hand (as presented in pendulum apparatus) and of upward and downward movement of the index-hand upon the other and traced the source of the latter to the relatively easier downward than upward movements of the eyes, Klemm working with the pendulum,¹ further isolated the two factors by alternately transposing the conditions of experimentation in such a manner, that the upward movement of the index-hand of the complication-pendulum was now coincident with retardation of the velocity, while conversely the downward movement was accompanied by an acceleration of the index-hand. This interchange of the relations of acceleration and retardation to the upward and downward movements of the index-hand, Klemm effected by inverting the medial position of the index-hand. This being

¹ Versuche mit dem Komplikationspendel nach der Methode der Selbsteinstellung. *Psychol. Studien*, ii, pp. 324-357.

in the original apparatus above, it was now below the center of the scale (the index-hand of Klemm's apparatus described nearly a complete circle), and therefore as the index-hand now approached the mid-point of the scale it moved in a downward course, with an increasing velocity: and as it left the mid-point it moved in an upward direction with a decreasing speed.

Now upon comparing points on the upward path of the index-hand in the transposed arrangement with similarly situated points on the downward path in the original arrangement, and *vice versa*, thus entirely excluding the influence of acceleration or of retardation, he found that the preponderant negativity still obtained upon the side of ascent; and upon a comparison of points upon the paths in the two arrangements, which points were identical as regards direction of movement and velocity, but at which was presented in the one case a phase of acceleration and in the other a phase of retardation, Klemm still found coincident with the phase of acceleration a greater negative tendency of the error as compared with the tendency occurring with retardation.¹

The especial interest of the experiments undertaken by Klemm lies in the altered method of procedure whereby the observer himself places a given mark in the position on an otherwise unmarked dial, at which the index seems to be when the sound enters. (The fundamental apparatus he employed is a modification of the original Wundt complication-pendulum.) In essentials the auxiliary device which this investigator used consists of a mirror, placed in front of the observer, between him and the dial. A circular opening through the mirror permits the observer to follow the movement of the shaft of the index-hand upon the dial, while the end of the index-hand, together with the scale is concealed from view by the mirror. The observer looks through a small circular opening in a screen, behind which he is seated. Upon the face of the screen opposite the mirror is a white annular surface the size of which corresponds to the graduated circumference of the dial, which

¹ It will be pointed out in Pt. III that Klemm's results on this point by no means indicate that the differences are really due to retardation and acceleration.

(the mirror being placed in a position equidistant from the dial and the white annular surface), is directly replaced by the reflection of the white ring—it being understood that the line of vision is perpendicular to the plane of the dial, reflected ring, mirror, its annular opening, and the hole through which the observer looks, at their centers. A second index-hand, called the marker, which may be moved radially over the white ring by means of a lever placed correspondingly to it upon the back of the screen, is thus under the control of the observer. With this arrangement therefore the observer who, through the opening in the mirror, follows directly the movement of the index-hand, may indicate its position in relation to the complicating stimulus, by adjusting the reflected marker to the position in which it is in alignment with the index-hand at the stroke of the sound-impression. Klemm used only two rates, 1.2 sec. and 2.1 sec. per oscillation. His farther problem was the determination of the width of the 'Simultaneitätsbereich,' the simultaneity zone, or region within which all positions are practically simultaneous with the discrete stimulus; and its relation to the average deviation or error. The main point brought out in this regard is the extreme variability of the region in question.

A simple complication-apparatus was devised by H. C. Stevens¹ through the use of a bell metronome. A piece of white card-board is fastened to the instrument in such a way that the pendulum oscillates in front of it. The card-board presents the arc of a circle, the radius of which is equal to the length of the pendulum, scaled in 5° intervals. The position of objective coincidence of the bell-stroke and pendulum, may be ascertained as in Wundt's machine, by slowly moving the pendulum across the dial until the bell sounds. The method of Stevens' experiments with this apparatus requires that the subject shall at one time direct his attention to the sound, in which case the sound appears to come *before* the position of objective coincidence (negative error), and at another that he direct his attention to the position of the pendu-

¹ A simple Complication Pendulum for Qualitative Work. *Amer. Jour. of Psych.*, xv, 1904, p. 581.

lum, in which case the sound appears to come after the real positions (positive error). This modification of method Titchener¹ calls the 'inversion' of the complication experiment, because in it the subject is not left unbiased but is expressly instructed to which member of the two simultaneously occurring stimuli he shall voluntarily direct his attention.

T. H. Haines² took up the complication problem in a new way. His apparatus consisted essentially of a black disc on which were mounted at regular intervals white letters, which, by the rotation of the disc were brought in succession before the field of view of an observation tube. This series of visual stimulations was complicated with an auditory impression produced by the stroke of an electric hammer. The circuit for this hammer was completed through a mercury cup by a copper wire attached to a wooden arm on the rotating shaft which bore the disc.

Haines used various rates of succession of the letters and of the sounds, varying them together and independently. His results agree in part with the Leipsic rule of change of average error with rate, and with the Angell and Pierce rule of change with increasing habituation. The fact that no correction was made for the errors in the apparatus, which were probably large at certain rates, makes the interpretation somewhat insecure. The method, however, seems a promising one.

Certain observations were common to all the experimenters. All noted the significance of accidental combinations of the attention in determining the judgments. Certain scale-divisions seemed to exercise a peculiar fascination upon the attention, and Wundt noticed that the attention invariably held to the scale-divisions, even when the actual position of entrance of the sound was made to occur midway between them and in cases in which the scale-divisions were separated by a considerable interval. Wundt performed experiments in which he concealed from view all but a single scale-division, and setting the position of the sound at points on one side and the other, he obtained errors as great as $\frac{1}{4}$ sec. in the time-displace-

¹ *Psychology of Feeling and Attention*, p. 252.

² *Harvard Psychological Studies* 1906, vol. ii, pp. 309-348.

ment. Geiger experimenting similarly obtained errors of as much as $\frac{1}{10}$ sec. It was also the general observation that a single revolution of the index-hand is inadequate to insure a conclusive judgment. For this it was necessary that a sense of rhythm in the succession of the momentary sound stimuli establish itself; so that several rotations were often requisite before the mind felt prepared to determine its choice. The difference in the number of preliminary rotations required, Geiger found to vary for his subjects between as few as three and as many as forty-three revolutions. Pflaum observed that the number of preliminary rotations required increased with the more rapid rates. If an increase occurred while the speed remained constant, it was an indication of the onset of fatigue.

II. NEW EXPERIMENTS AND RESULTS.

In the work just reported the assumption has been implicitly and sometimes explicitly made that the time-displacement of the discrete stimulus in relation to the series of other stimuli is essentially a complication phenomenon, *i. e.*, that it depends upon the introduction of a disparate impression into the series.

In the paragraph of Wundt's *Physiologische Psychologie* introductory to his description of the complication experiment,¹ he applies the name 'time displacements' to such time illusions as occur when simultaneous sense impressions or sense impressions but slightly discrepant in time appear to be displaced in relation to each other in such wise that impressions which are in truth simultaneous are perceived as successive, or where a succession is actually present, the impressions are perceived as simultaneous or even transposed so that the earlier is perceived as the later and the later as the earlier. But Wundt insists upon the essential disparateness of the two impressions as the *sine qua non* of such displacements and there is in the text the obvious implication that this stipulation applies equally under the specific conditions of the complication experiment. "Such time-displacements," he says, "occur essentially between disparate sense impressions, that is, between visual and audi-

¹ 5th ed., p. 4.

tory, visual and tactual, and between tactual and auditory. They happen, when the impressions are within the same sense-realm, only when the impressions affect different individual organs, as the two eyes or ears, or positions upon one and the same organ which are widely removed from each other, for example, upon the skin of the upper and of the lower extremity. That stimuli upon the same ear or eye, or upon one and the same position of the skin should be temporally displaced with regard to each other, is, on the contrary, never to be observed."

Wundt then attributes the chief source of the phenomenon of time-displacements to the preference of the attention for stimuli received upon sense-organs in which the physiological transmission is the more rapid, claiming upon this ground, that the attention seizes upon an auditory impression more readily than upon a visual one, because of the conditions for the quicker reception of a stimulus in this sense organ.

He then argues that the sense organ in which stimuli are more favored by the attention is, *de facto*, the sense organ which will find most favor in the process of 'apperception.' It was primarily with a view to testing the validity of the above quoted statement of Wundt, and the explanation of time-displacements hinging upon it, together with the *a priori* assumptions Wundt adduces in support of it, that my experiments were undertaken.

To subject to a critical experimental test the question whether time-displacements may occur where the two impressions belong to the same sense organ and are received upon adjacent portions of it, I substituted for the disparate auditory stimulus entering momentarily into the course of the series of visual impressions as presented in Wundt's complication experiment, a momentary visual impression.

Before describing the technique of my procedure, I wish to call attention to certain features of the apparatus I employed, in which it possesses a distinct advantage over the instruments hitherto used.

As I have said, the dial of Wundt's machine was marked with scale-divisions. Those were clearly seen by the observer

and by means of them he indicated his judgment of the position. In another arrangement, which Geiger adopted for the purposes of a variation of his experiment, the index-hand traverses a surface which is unmarked throughout its extent. Obviating the objections to which both these arrangements are open, a characteristic feature of my experiment is the introduction of the device whereby the subject may himself shift the temporal position of the discrete stimulus until it appears simultaneous with the alignment of the index-hand and a determinate mark or goal upon an otherwise undifferentiated dial. It will be recalled that Klemm employed the method by which the subject uses an adjustable marker to denote the position at which he judges the two impressions to be simultaneous, but by this method of self-fixation it is the marker which is brought to the position of entry of the discrete stimulus. This is doubtless a convenient method of locating the position of subjective simultaneity, and permits the determination of the limits of the region of simultaneity, but it does not otherwise alter the essential conditions of the experiment.

My apparatus had a semicircular dial, upon which appeared a single black mark (called the *goal*), placed at the middle point. The pointer moved over this dial and by this mark at uniform rates, while the temporal position of the discrete stimulus with regard to the series of positions of the index-hand could be varied at will by the movement of a lever. The subject was required to fix his eyes on the goal and vary the aforesaid temporal position, until it coincided with the alignment of index-hand and goal, starting alternately from positions much earlier and much later.

The advantage of the method I adopted is three-fold. In the first place it is to be preferred to the method of determining the position by the scale-divisions of the graduated dial, because it eliminates the random influence upon the attention of certain of the scale-divisions and concentrates the attention uniformly upon a single mark, which the index-hand is to reach simultaneously with the entrance of the disparate impression. Secondly, it takes precedence over the method whereby the subject selects the position in question upon an entirely

unmarked dial, for the reason that there is in this case no mark which may serve the attention as a fixation-point to which it may attach itself as an aid to the subject in determining his judgment, or upon which he may depend for reference in designating subsequently the selected position, without involving the element of memory. And lastly and perhaps of chief importance among the features essential to the method to which my apparatus is adapted is the elimination of eye-movements around or across the dial. For here the arrangement is precisely the opposite of that introduced by Klemm, and instead of an adjustable marker being brought to the position of apparent simultaneity of the impressions, the momentary stimulus is brought, as it were, to the marker or goal at the instant of the arrival there of the index-hand—the discrete stimulus being thus made simultaneous with the alignment of the index-hand and the goal. Thus the goal at which the disparate impressions are to be brought simultaneously is the focus of interest. Upon this the eyes are steadily fixated, instead of having to shift constantly to catch the position at which the stimulus enters, as in the method of Wundt and others.¹ Finally, my method shows a further departure from the routines adopted heretofore (with the exception of Klemm's) in that it presents the factor of two opposite directions of approach, *i. e.*, from definite positions on the side, where the stimulus clearly comes before the alignment and from definite positions on the side, where alignment precedes stimulus, thus neutralizing in the total of the results of the two types the influences due to direction of approach.

My apparatus was constructed from a Schumann time-machine² in the following way. A metal disc *M*, 25 cm. in radius, bored through the center to fit the shaft *S* of the time-machine, was slipped over the latter and rested horizontally upon the pulley *P*. Resting upon the metal disc was a corre-

¹ While the apparatus employed by me is similar to Klemm's in the introduction of the method of self-fixation, it was devised entirely independently. My experiments had continued for some months prior to the appearance of Klemm's article, in which is presented a description of his device.

² *Zeitschr. f. Psychol.*, Bd. 17. S. 253. See Catalog of Spindler & Hoyer for cut.

sponding disc of white card-board *D*. Both discs rotated with the shaft and with the contact arm *T*, but the position of the card-board disc and therefore of the index-hand could easily be changed. Upon the card-board disc was pasted the index-hand *H*, which consisted of a black strip, 11 cm. in length, lying upon a radius and terminating at the edge of the disc. The strip was wedge-shaped, its apex at the edge of the disc being 2 mm. in width, and its base 14 mm. In the horizontal plane with the card-board disc was a card-board dial, presenting an originally rectangular surface, in one edge of which, equidistant from either end, was niched a semi-circle having a radius slightly greater than that of the disc and placed concentrically and nearly touching the disc. The disc therefore rotated in close proximity to the concave edge. From the center of this concave edge, a scale was drawn outward 75° toward either side, the scale-divisions, drawn lightly with pencil at 5° intervals, being only with difficulty visible to the observer. At the mid-point for a mark or goal *G* was pasted a strip of black paper similar to the index-hand, but having a length of only 6 cm., and width tapering from 1 cm. to 3 mm. This strip was placed radially, its narrowest end at the edge of the card-board.

The circular brass scale *N*, beneath the revolving parts of the time-machine was cleared of all impedimenta save the single contact-block which moves with the lever *L*. A mercury contact *RQ* was employed when working with a light-stimulus, and a metallic contact in the case of the sound-work.

A small Zimmerman sound-hammer placed directly beneath the center of the dial served for the auditory-stimulus. For the light-stimulus I used the flash of a Geissler tube *E*, 12 cm. in length, placed horizontally about 12.5 cm. above the center of the dial and actuated by an induction coil. The resistance of the flash-circuit was so great that the current did not jump across at the make of the primary circuit. There was therefore only one flash, namely, that caused by the break of the primary circuit. A condenser of sufficient capacity, in parallel with the break, prevented any disturbing sparking

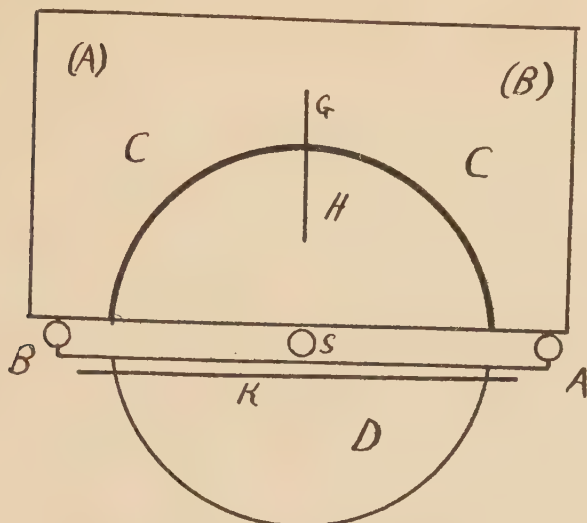


Fig. 1. Horizontal plan of disc and dial.

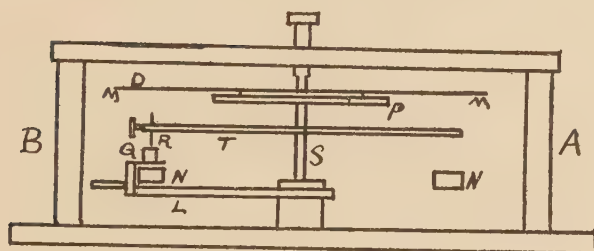


Fig. 2. Cross-section.

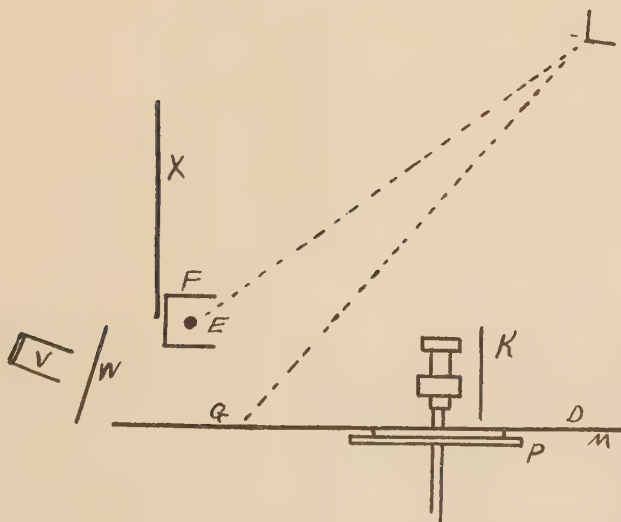


Fig. 3. Cross-section.

at this point. The flash in the Geissler tube was entirely inaudible to the subject. The tube was suspended in an oblong card-board box *F* ($20 \times 7.5 \times 7.5$ cm.), the anterior side of which was wanting. The interior surface of the box was black. The box was used in order to secure a more vivid flash, by intercepting the background of light which illumined the field of operation, and also to prevent the light from falling upon the dial and disc; which would have made an instantaneous photograph on the retina of the position of the index-hand. A screen *X* of black card-board above the box containing the Geissler tube shielded the eyes of the observer from the incandescent lamp *V* serving as constant source of illumination. The light of the latter was diffused by means of a plate of ground glass *W* placed vertically in front of it.

The machine was rotated by a belt transmission from an electric-motor which gathered impetus very quickly and maintained speeds of great uniformity, which were accurately controllable by means of a rheostat. The four rates I selected were one revolution in 2.25, 1.75, 1.25, and 0.75 seconds. The rates were obtained by observing the second-hand of a watch, until, through appropriate adjustment of the strength of the current in the rheostat there were four flashes (or clicks) in 9, 7, 5, and 3 seconds, according to the rate employed, the counting being continued in any one observation over five or six times these intervals. A key in the primary circuit enabled the experimenter to omit the flash or click at will.

The table upon which the time-machine rested was of such a height that the white disc was about upon a level with the eyes of the observer when seated. A screen *K*, 6 cm. in height and of the length of the diameter of the disc, placed just back of the center of the latter, intercepted the observer's view in the intervals of experiment. The long axis of the machine extending between the two vertical brass pillars was at right angles to the direction in which the observer faced, hence the finger lever could be moved in a clear sweep from pillar to pillar or through an arc of about 150° . For convenience of

designation, the side of the circular brass dial at the right of the observer was called the 'A' side, and so denoted by the letter *A* placed at the right-hand end of the path, and correspondingly the side of the brass dial at the observer's left hand was called the 'B' side and marked accordingly. Correspondingly for the experimenter who stood opposite the observer the quadrants on the white card-board scale on his right and left were labeled the 'A' and 'B' quadrants respectively.

The method of procedure was as follows: the room was darkened except for the incandescent lamp serving to illumine the dial and the disc over its anterior half. The experimenter called 'fix "A"' (or 'fix "B"'), whereupon the observer moved the finger-lever to the *A* (or *B*) side, in the neighborhood of the right (or left) column, and rotated the contact-arm *T* until, in the case of the auditory stimulus, the metallic strip attached to the arm made contact with the center of the surface of the metallic block, attached to the finger-lever; or, in the case of the visual stimulus, until the needle *R* stood at the middle of the substituted cup *Q*, (the circuit during this procedure being of course interrupted by the key as above described). The revolving arm being held in this position, the experimenter shifted the card-board disc until the index-hand was brought to the desired position, as indicated by the numbered scale-divisions in the quadrant of the dial diagonally opposite to the position of the lever—in the 'A' quadrant if the lever is at the 'A' side, and in the 'B' quadrant if the lever is at the 'B' side. The experimenter then said 'Ready,' at which signal the observer released the contact-arm, and the experimenter closed the switch that started the motor, setting the machine in motion. When a uniform speed had been attained, for which three or four revolutions of the disc sufficed, the experimenter said 'now' and closed the key controlling the discrete stimulus. At 'now' the subject rose and with hand on finger-lever, fixating the goal, observed (by indirect vision) whether the disparate impression occurred synchronously with the passage of the index-hand, at a position within the 'A' quadrant, or at a position in the 'B' quadrant, *i. e.*, whether the complicating stimulus occurred at a position in front of the goal,

relative to the direction of rotation of the disc, as in the case of 'A' settings, or at a position behind the goal as in the case of 'B' settings. Then, still steadily fixating the central or zero position indicated by the goal, he moved the finger-lever and hence the position of the discrete stimulus in the path of the index-hand until, by advancing or retracting it, as necessary, the stimulus appeared simultaneous with the alignment of the index-hand and the goal. When satisfied that the object had been attained, the observer so indicated, and took his seat, whereupon the experimenter opened the switch controlling the stimulus-circuit and the switch controlling the motor. At the word 'set' the observer without moving the finger-lever rotated the arm until contact was made as before, enabling the experimenter to take the reading by the index-hand which was thus set at the position of judgment. The position being recorded, the procedure was repeated as before.

The technique specifically adopted called for the alternate shifting of the finger-lever, and hence of the stimulus, from 'A' to 'B.' Beginning each experiment-period with 'A,' six successive judgments were made with each rate, that is three upon the 'A' side and three upon the 'B' side, and with a uniformity in the order of the positions at which the preliminary setting was made in each trial. From side to side the positions were (in degrees)

A	B
60°	45°
75°	60°
45°	75°

Thus was obtained a perfectly definite order of starting points, which yet seemed to the subject hap-hazard as regards their distance from the goal. The above order was selected with a view to securing the utmost possible balance between the two sides.

Further, this provision for irregularly alternating succession of positions upon the 'A' and 'B' sides was made, along with the instruction to shift the lever 'carelessly' *i. e.*, avoid-

ing returning it always to exactly the same position on either side, as a precaution against prejudicing the observer's results through his discovering any rigid method of setting out, and so inspiring in him an anticipatory feeling of the length of arc to be traversed by the discrete stimulus in order to bring it to the goal simultaneously with the index-hand.

With regard to the designation of the direction of error I follow precedent of Wundt and others, by designating as positive those errors in which the assumed position is later than the real position; and as negative, the errors in which the assumed position is earlier than the real one. Wundt read therefore from actual to assumed position in every case. Accordingly if the index-position which the subject judged to be at zero was really in the 'A' quadrant, as shown in the reading from the 'set' position, the error was positive; if the index-position which the subject judged to be at zero was really in the 'B' quadrant, the error was negative.

With regard to the order of the succession of the velocities: this was taken in three ways. In the first I began with the fastest rate and proceeded to the slowest; in the second I proceeded in the reverse direction, beginning with the slowest and gradually going over to the fastest; and thirdly the succession of rates was arranged in such a way that the rates intermediate in speed were made the extremes in the order of succession, and *vice versa*, so that the series was now 1.25, 0.75, 2.25, and 1.75.

With the light stimulus it was necessary to make a correction for the lag of the stimulus, due to the drag of the mercury in the cup, *plus* the latency of the spark-coil and Geissler tube, which together amounted, with the different rates, from fastest to slowest, to 2σ , 6σ , 8σ , and 14σ , respectively, to be subtracted from the results. This correction was obtained by turning out the light which normally illumined the dial, and inverting the box containing the Geissler tube, so that the tube was hidden from the eye, and the flash illumined the dial and the index-hand. The index-hand was set by the usual method to read zero at the moment of the flash, and then put in motion at the speed for which the error was to be determined. In

this way a photographic image of the actual position of the index-hand at the moment of the flash was produced on the retina and the deviation from the zero point could be readily noted. Black dots placed upon the scale-divisions facilitated the reading of the actual position due to the lag. The lag of the Geissler tube and spark-coil was independently determined by physical methods and found to be never greater than 1σ . The greater part of the lag was therefore due to the mercury-cup itself. No method is available for determining the latency of the physiological process in the retina.

Likewise to allow for the lag of the hammer employed for the sound stimulus, it was necessary to subtract 6σ , 5σ , 3σ and 2σ from the results obtained with the rates from fastest to slowest, respectively. These lags were accurately determined from kymographic records of the movement of the hammer and a tuning-fork curve, together with the time elapsing at each rate between the first making of contact and the instant of reaching the central point on the contact-block at which the readings were taken throughout the experiment. Further, account was taken of the time required for the sound from the hammer to reach the ear. As the hammer was approximately three feet from the subject's ear, this was assumed to be 3σ .

The corrected results of the first portion of my experimental work are given in part in Tables 1 and 2. Table 1 gives the average positions, *i. e.*, the averages of the displacements of the judged positions for each individual subject and rate for the sides 'A' and 'B' and for 'A' and 'B' taken together. The first column under 'subject' gives the five subjects by number. The second column 'rate' gives the seconds per rotation of the rates to which the figures correspond. The third column 'A' gives the average displacements when moving from the 'A' side, *i. e.*, when the flash was, at the beginning of the single experiment, much too early. The fourth column 'B' gives the average displacements, when moving the flash from the reverse or 'B' side. The fifth column 'total' gives the averages of the figures in the 'A' and 'B' columns. The sixth, seventh, and eighth columns give for the experiments

with sound the averages corresponding to those in the third, fourth, and fifth columns for light.

Table 2 gives the average variations corresponding to the averages in the 'A' and 'B' columns of Table I. No average variations are given corresponding to the 'total' columns in Table I, because the only significant figures for such purpose

TABLE I.

Subj.	Rate	LIGHT.			SOUND.		
		A	B	Total	A	B	Total
I.	0.75	+ 0.4	-21.0	-10.3	-23.3	-22.9	-23.1
	1.25	-14.0	-41.2	-27.6	-46.0	-51.2	-48.6
	1.75	-10.4	-49.3	-29.8	-36.2	-45.1	-40.6
	2.25	- 4.7	-40.0	-22.3	-23.8	-38.4	-31.1
II.	0.75	+ 9.4	+22.6	+16.0	- 0.2	+15.1	+ 7.4
	1.25	- 2.0	+ 2.6	+ 0.3	- 2.2	+ 3.6	+ 0.7
	1.75	-23.3	-18.5	-20.9	+ 3.4	+ 9.1	+ 6.2
	2.25	-18.1	-10.9	-14.5	+ 3.2	+11.5	+ 7.3
III.	0.75	+16.0	-17.9	- 0.9	+22.0	+12.3	+17.1
	1.25	+26.9	+ 0.3	+13.6	+32.5	+27.9	+30.2
	1.75	+24.4	+ 0.9	+12.6	+37.5	+37.5	+37.5
	2.25	+16.2	- 6.8	+ 4.7	+33.4	+33.4	+33.4
IV.	0.75	- 5.1	+ 0.4	- 2.3	-33.7	-40.3	-37.0
	1.25	+ 3.8	- 7.1	- 1.6	-28.7	-41.4	-35.0
	1.75	+ 4.1	+ 9.0	+ 6.5	-30.3	-29.5	-29.9
	2.25	+ 2.6	+ 0.5	+ 1.5	-35.3	-32.2	-33.7
V.	0.75	-31.4	-37.7	-34.5	-15.3	-24.0	-19.6
	1.25	-44.7	-62.6	-53.6	-45.4	-61.0	-53.2
	1.75	-28.2	-25.0	-26.7	-53.2	-37.8	-45.5
	2.25	-40.0	-43.1	-41.5	-68.6	-49.9	-59.2

would be the average of the variations for 'A' and 'B', the 'A' and 'B' averages in Table I forming distinct groups with variations about independent centers of equilibrium, as will be more fully brought out. If the average variation from the total were taken in the ordinary way, it would simply be adding the variable error to the constant error which is an entirely

unwarranted proceeding. The variations are given in their absolute values, not relative to the average displacement, because the measure of the average displacement is simply a convenient way of denoting a position on an arc of a circle by its distance from an arbitrarily fixed point. Other things

TABLE 2.
Average Variations.

Subj.	Rates	LIGHT		SOUND	
		A	B	A	B
I.	0.75	28.4	21.4	16.9	19.5
	1.25	23.1	22.4	10.9	16.7
	1.75	23.4	16.2	10.5	13.7
	2.25	16.6	19.7	14.5	17.7
II.	0.75	16.9	16.9	10.0	12.8
	1.25	12.7	15.0	8.9	8.6
	1.75	7.2	15.3	7.2	8.1
	2.25	19.7	23.5	8.3	7.2
III.	0.75	15.6	21.4	19.0	12.8
	1.25	18.5	15.6	12.1	17.9
	1.75	20.2	16.2	11.3	17.0
	2.25	15.6	25.0	13.5	17.7
IV.	0.75	16.9	16.9	8.6	14.2
	1.25	13.3	16.1	9.2	11.5
	1.75	20.2	13.7	6.4	10.5
	2.25	15.6	17.7	10.4	8.3
V.	0.75	27.3	31.8	22.5	23.2
	1.25	25.4	27.1	19.0	31.8
	1.75	32.4	33.2	18.6	33.2
	2.25	30.2	27.8	26.0	29.1

being equal, we should have as much reason to expect a variable error of a given magnitude at one point on the arc as at another. It is obvious that if the average variation were expressed in percentages or in some such way in relation to the magnitude of the displacement, a displacement of zero (which might readily occur, as seen by the value of 0.4 in two cases) would

necessarily make the number representing the variable error infinitely large, which furnishes the *reductio ad absurdum* for the proportional statement of variable error and constant error. All values are expressed in σ throughout. Other experimenters fail to include in their results a table of average variations. The omissions are unfortunate, for a comparison of the average variations of my subjects with those of the subjects of the other experimenters would be interesting.

Five persons served as subjects for my investigation: Prof. G. M. Stratton, Dr. Knight Dunlap, Dr. W. D. Furry, Mr. T. A. Lewis, and myself. The experiments here reported were conducted for a period of two years, from the spring of 1907 to the spring of 1909.

My tables are constructed on the basis of the averages of 72 judgments for each subject, with each of the four rates employed, of which 36 are judgments made in the 'A' quadrant and 36 made in the 'B' quadrant.

First to be remarked is the fact that displacements, both positive and negative, occur when a momentary *light* impression is substituted for the discrete auditory stimulus hitherto employed in the complication experiment. In general the displacement with the visual stimulus is numerically less than with the auditory. The only marked exception is the case of Subject II, whose errors for each rate but one are greater with the light than with the sound impression. It is further seen that in general the direction of the displacement is the same for a given subject with both forms of stimuli, the exception being Subject IV, whose errors are all negative with the sound-stimulus, while with the light-stimulus his positive errors predominate. It is noteworthy that for each subject, with the sound, the direction of the displacement is consistently the same for each of the eight groups of judgments, (the exception appearing in the case of Subject II, whose displacement is negative in direction for the 0.75 and 1.25 sec. rates in the 'A' quadrant, all the other averages being positive) while for light there is among the groups corresponding to different rates considerable discrepancy as to the quality of the error.

With regard to the effect of the rate of rotation of the index-

hand, and consequent rapidity of the succession of the discrete impressions: the relative displacement for the four rates employed differs with different subjects in amount and direction. For example, with Subjects I and II, the tendency is toward a more negative displacement with the slower rates, which accords with the results of Wundt and the other German investigators, while on the other hand, in the case of Subjects III and IV, a directly opposite tendency is shown, the lower rates producing a more positive result. With Subject V, no uniformity is to be traced in this respect. Further, the relative magnitude of the displacement for different rates, disregarding now the sign, varies with different subjects. For example, with the light-stimulus, the magnitude of the errors of Subject I are least with the 0.75 sec. rate, while the least error with Subject IV is at the 2.25 sec. rate, and for Subject V, at the 1.75 rate.

Considering now the results of 'A' and 'B', a comparison of the error obtained in each pair in the case of Subjects I, III, and V shows that the tendency of judgments, made when the position of starting out is in the 'A' quadrant, is positive in relation to 'B', that is to say that, in moving from 'A' or 'B', the tendency is to stop on the corresponding side of the average position of the two.

Comparing the errors of 'A' and 'B' in the case of Subject II, gives a result which is just the reverse of that given in the three cases cited. This discrepancy is only apparent however and the explanation is to be sought in the reversed procedure, which is characteristic of this subject's method in making a determination and in accordance with which the 'A' judgments and the 'B' judgments became virtually transposed. For in every judgment it was the practice of this subject to continue shifting the temporal position of the discrete stimulus from the starting position either before or after the alignment of index-hand and goal, until he found that he had overstepped the position of simultaneity, and hence was obliged to return to it. So that in setting out from the 'A' side, let us say, he invariably passed over to the 'B' side of the scale, until assured of a discrepancy, when he would finally set about making the

discrete stimulus synchronous with the alignment—hence a ‘B’ judgment. In setting out from the ‘B’ side, his procedure was similar; the ultimate judgment becoming in reality an ‘A’ judgment. This explanation was not elicited from the subject after noticing the variance of his results. His manner of procedure had caught my attention from the first, without my seeing in it at the time any ulterior significance. Later, the subject himself, during the course of an experiment, voluntarily called attention to the fact that his procedure was invariably the reverse of the normal method above described. The subject was of course entirely in ignorance of his results when he made this statement. In the light of this explanation, the results which Subject II show with regard to the ‘A’ and ‘B’ direction of approach are entirely consistent with the results of the other subjects, so that the tendency of the ‘A’ judgment to stop on the ‘A’ or positive side of the average position of ‘A’ and ‘B’, and of the ‘B’ judgments to stop on the ‘B’ or negative side of the average position of the two is characteristic in general.

It may be noted that with Subjects I and III, the interval between the values of ‘A’ and ‘B’, that is, the distance of ‘A’ and ‘B’ from the average of both is much greater in the case of the Light impression than for Sound. For Subject II, the influence of the new position of setting-out (that is, reversing the direction after having crossed the goal) cannot but be off-set in a measure by the counter-effect of the influence of the original starting-point. Thus the effects of the two influences are composed in the results for Subject II, and probably account for the small separation of the ‘A’ and ‘B’ averages in his case. Subject IV presents results also less regular in this regard, particularly where the stimulus used was the light, but the explanation turns upon the same point as in the case of II, it having been also (though not invariably) the practice of this subject to over-reach the goal and then return to it.

An important factor which my experiments bring to light is the influence of the position of setting out upon the direction and magnitude of the final judgment. The effect is specifically shown by a comparison of the different results obtained,

when setting out from different determined positions upon either side of the goal. To test this influence I compared for each subject the averages of the twelve judgments, obtained with each rate with the different positions of origin; that is, when starting out from the position of 75° , 60° , and 45° on the 'A' side and when starting out from the corresponding positions on the 'B' side. This comparison was made in the case of both light and sound stimuli. The results are given in Tables 3 and 4, for light and sound respectively. The averages are here given in degrees. The tables show the

TABLE 3.

Light.

STARTING POSITIONS. IN DEGREES.

Subj.	Rates.	A.			B.		
		75°	60°	45°	75°	60°	45°
I.	0.75	+15.80	+ 3.10	-12.30	-18.4	-12.10	+ 1.5
	1.25	- 1.7	- 3.8	-13.1	-17.0	-11.9	- 3.9
	1.75	+ 1.7	+ 2.0	- 6.5	-11.3	- 6.4	- 5.0
	2.25	+ 2.5	+ 3.6	+ 1.4	- 5.4	- 3.8	- 2.1
II.	0.75	+11.83	+ 0.66	- 2.08	+ 8.58	+12.75	+14.08
	1.25	+ 0.33	+ 1.5	- 1.25	+ 5.0	+ 1.25	+ 0.16
	1.75	- 3.25	- 4.25	- 3.75	- 3.83	- 0.83	- 1.5
	2.25	- 0.41	+ 0.16	- 2.66	+ 0.25	- 0.83	+ 1.0
III.	0.75	+18.25	+10.66	+ 0.25	-18.08	- 8.91	- 0.16
	1.25	+12.91	+10.33	+ 2.66	- 1.5	+ 1.75	+ 5.48
	1.75	+11.5	+ 6.0	+ 1.75	- 0.75	+ 1.91	+ 5.16
	2.25	+ 5.58	+ 6.5	+ 2.91	- 1.34	+ 2.5	+ 2.16
IV.	0.75	+ 3.0	+ 2.16	- 6.25	- 7.33	- 0.33	+ 7.33
	1.25	+ 4.08	- 0.48	+ 3.91	- 0.58	+ 2.0	+ 0.58
	1.75	+ 4.83	+ 2.5	- 0.16	+ 2.91	+ 3.58	+ 5.5
	2.25	+ 4.5	+ 1.75	+ 0.91	+ 2.25	+ 4.75	+ 1.58
V.	0.75	- 1.33	-16.33	-28.25	-31.5	-23.66	- 5.0
	1.25	- 6.66	-12.25	-19.41	-22.16	-18.91	- 7.41
	1.75	+ 0.16	- 4.33	-10.25	- 7.41	- 4.5	+ 1.91
	2.25	- 2.33	- 3.16	- 6.58	- 6.66	- 8.0	+ 0.66

TABLE 4.

Sound.

STARTING POSITION. IN DEGREES.

Subj.	Rates.	A.			B.		
		75°	60°	45°	75°	60°	45°
I.	0.75	- 1.91	- 8.0	- 8.75	-18.41	- 7.08	- 2.83
	1.25	-11.08	-14.33	-12.25	-15.91	-12.41	-10.00
	1.75	- 5.08	- 5.16	- 7.83	- 9.91	- 7.41	- 7.75
	2.25	- 2.66	- 2.75	- 4.25	- 8.5	- 5.41	- 4.25
II.	0.75	+ 5.58	+ 5.41	+ 1.0	+ 5.5	+10.16	+18.25
	1.25	+ 3.33	+ 0.5	+ 1.33	+ 0.75	+ 3.83	+ 5.08
	1.75	+ 3.08	+ 1.16	+ 0.83	+ 1.91	+ 3.25	+ 4.25
	2.25	+ 3.16	+ 0.16	+ 0.33	+ 1.08	+ 2.66	+ 3.75
III.	0.75	+21.25	+15.25	+ 6.0	+ 2.83	+12.08	+14.66
	1.25	+14.41	+12.16	+10.25	+ 4.91	+11.0	+16.75
	1.75	+11.33	+ 9.0	+ 7.41	+ 6.08	+10.5	+12.5
	2.25	+ 8.83	+ 5.08	+ 5.5	+ 3.58	+ 7.0	+ 8.91
IV.	0.75	-13.66	-13.91	-16.5	-23.58	-17.08	-12.58
	1.25	- 6.41	- 5.41	-10.91	-10.66	-10.91	- 8.58
	1.75	- 6.0	- 5.33	- 6.83	- 7.33	- 7.0	- 7.08
	2.25	- 4.91	- 6.16	- 4.91	- 6.58	- 3.83	- 5.91
V.	0.75	+ 0.75	- 2.66	-19.83	-18.58	-10.5	- 2.08
	1.25	- 9.33	-10.83	-18.75	-28.0	-11.75	- 6.66
	1.75	- 9.41	- 7.0	-14.5	-12.25	- 8.33	- 1.08
	2.25	-10.16	- 9.0	-12.5	- 9.83	- 7.41	- 4.08

remarkable regularity of the influence of the starting point in the coincident progression of final judgments and original positions, the final judgments of the 'B' side becoming progressively more positive as the starting point is shifted in a positive direction and correspondingly the final judgments of the 'A' side assuming a more negative position parallel with the more negative shift of the initial setting of the index-hand.

Because of the resulting feebler contact as the rates became progressively faster, the intensity of the stimulus diminished with increasing rates. As this factor introduced the question whether the varying results obtained with different rates were

due to the corresponding variation in the intensity of the stimulus, new experiments were undertaken with a view to studying the influence of the intensity of the discrete stimulus. Using Subjects I, II, III and V, I obtained with each on three different days, eighteen judgments with a weak intensity, and the same number with a stronger intensity, the eighteen judgments consisting in both cases of nine of 'A' and nine of 'B.' Sound was used throughout this variation of my experiment. I proceeded by giving groups of six trials, that is, three each of 'A' and 'B,' and alternating the intensity with successive groups. The two intensities were designated 'Weak' and 'Strong,' the weak being the weakest stroke employed in the experiments, and the strong consisting of a very loud, sharp stroke. The results as shown in Table 5 were practically negative, the only considerable difference occurring in the averages for Subject V, whose determinations were always erratic. The values in Table 5 are given in σ , and are the excess of the average errors with 'Strong' stimulus over the average error with 'Weak.'

TABLE 5.
Intensity. Sound. Rate 1.25.
Strong minus Weak.

Subj.	A.	B.	Total.
I.	- 3.47	+ 3.47	0.0
II.	+ 0.01	+ 3.47	+1.74
III.	0.0	0.0	0.0
V.	-10.41	+17.35	+3.47

Since along with Wundt's denial of the possibility of obtaining a displacement in cases in which two momentary stimuli are of the same sense, he excepts the cases in which the conditions make possible the wide separation of the two stimuli, as upon the upper extremity and the lower extremity, or with one and another of a pair of organs of the same sense, as with first one eye and then the other; a series of experiments were now made in which the two visual impressions, the flash and the passing of the index-hand, fell upon contiguous areas of

the retina. The reader will recall that while, in the first experiments, the impressions were on adjacent portions of the retina, they were not strictly contiguous, the Geissler tube being separated from the goal (and its neighboring regions) by an interval of approximately 12.5 cms. The condition of actual contiguity was satisfied by inverting the box containing the Geissler tube, so that the flash fell upon the dial, while the subject did not see the tube at all; the margin of the area illuminated by the flash coinciding with the edge of the dial and hence of the scale, along which moved the index-hand. The flash was limited to the dial in this manner, and not permitted to fall upon the index-hand, for in such a case a retinal photographic image of the actual place of the index-hand would have been produced, thus precluding the conditions essential to the conduct of the experiment. The results of this experiment are given in Table 6. Each number given under 'A' and 'B' in this table expresses in σ the average of twenty-four judgments. As the results show, where conditions are introduced which cause contiguous visual images to fall upon the retina, a time-displacement occurs not differing in direction from the time-displacement observed where the stimuli were allowed to fall upon nerve-terminals of the retina which were more remote from each other.

TABLE 6.
Contiguous Visual Stimuli. Rate 1.25.

Subj.	A.	B.	Total.
I.	-10.05	- 42.5	-26.1
III.	+49.04	+ 23.0	+36.02
IV.	- 9.85	- 32.6	-21.22
V.	- 7.45	-103.05	-55.25

In the case of Subject I, the magnitude of displacement is about the same in the two cases. In the cases of III and IV the magnitude is greater with the contiguous stimulation; and in the case of V, although the magnitude of the total is about the same, 'A' and 'B' are farther apart.

To test the validity of the alternative requirement which Wundt makes as a condition of obtaining a temporal displacement in the case of two simultaneous impressions belonging to the same sense-realms, namely, that they occur each upon one of a pair of organs of the same sense, I conducted a few experiments in which the subject observed with monocular vision.

Performing a series of experiments upon different days on Subjects I and IV, and having them alternately use the right and left eye, I again obtained results, agreeing essentially with the readings obtained when the subjects presumably employed binocular vision, but showing a more exaggerated displacement. If, however, it be suggested that even when both eyes were open, the subjects really employed monocular vision, the results are in no wise affected.

The general method of my experiments, *i. e.*, the active approximation, by the subject, of the discrete stimulus to a fixed position of the index-hand, introduces, as has been clearly brought out above, factors which are not explicitly present in the judgments obtained by the older methods, although it is by no means certain that they are not operative in those cases. In order to find whether the general averages, in which these factors may be supposed to compensate for each other, differ much from results obtained by the older methods, further experiments were undertaken with Subjects I, III, and V, in accordance with the Wundt procedure. For this purpose another scale was made which did not bear the black strip at the zero point to indicate the goal, but bore instead the scale drawn in black lines 13.5 mm. long, and 1 mm. wide, five degrees apart, and extending 75° in both directions from the central point. For accuracy in recording, these were numbered in pencil, but so faintly as to be indiscernible to the subject. Following the classic methods the experimenter placed the discrete stimulus at a position of the index-hand unknown to the observer, and when the latter had declared the position at which the index-hand seemed to be, at the moment of the discrete stimulus, the two positions were compared and the difference between them, *i. e.*, the magnitude of the displace-

ment, if any, recorded; the quality of the displacement as usual being called positive if assumed position comes *after* the real one, and negative if the apparent position comes *before* the actual position.

In these tests, the series used, in the experimental hour, consisted of nineteen different 'settings' of which nine were at different positions in the 'A' quadrant and nine at different positions in the 'B' quadrant, the other position being at zero. Only one rate was used, the 1.25. The discrete stimulus was necessarily sound. There was but one series given in the case of Subject I, and for Subjects III and V, two series each were obtained. The results are given in σ in Table 7, each number under (A) and (B) being the average of nine determinations, namely, the nine given in the quadrants designated as A and B respectively in the previous experiments.

TABLE 7.
Passive Method. Rate 1.25.

Subj.	(A)	(B)	Av.
I.	+20.82	- 41.64	-10.82
III. (1)	+34.7	- 27.76	+ 3.47
(2)	+38.17	- 48.58	- 5.20
V. (1)	-45.11	- 90.22	-67.67
(2)	-65.93	-100.63	-83.28

For Subject I, the displacements for the settings in the 'A' quadrant were all positive, and in the 'B' quadrant all were negative. The same was true for Subject III, with three exceptions in the 'A' quadrant, although the average was there positive. Subject V is so far in agreement with the other two that the displacements are more negative for the B quadrant.

III. DISCUSSION AND INTERPRETATION.

The experiments which I conducted after the method of Wundt (see Table 7) brought out a result strikingly different from what was found by Wundt and Geiger. This result was the displacement of the apparent simultaneity in opposite direc-

tions on the two halves of the dial, the errors being almost all positive in the 'A' quadrant and negative in the 'B' quadrant. It is true that the German experimenters had found a difference in this direction between the results for the two halves of the dial, explained by them as due to eye-movements; but the difference they found was merely a greater positivity (or less negativity) on the one side as compared with the other, and not a complete reversal with considerable magnitudes.

Now this opposition between the results given in the 'A' and 'B' quadrants is obviously the outcome of some feature of my experiment, which is not present in the experiments of Wundt and others. It will be remembered that in this part of my work the dial presented to the subject was uniformly scaled throughout, there being no conspicuous mark to indicate the middle point of the dial, as was the case in my other experiments.

This difference between the errors in the two quadrants agrees in kind with the difference between the errors when moving from 'A,' and the errors when moving from 'B' in the preceding experiments on the same observers, although in these preceding experiments the factor of eye-movement is either excluded or at least reduced to minimal terms. It is therefore impossible to escape the conclusion that the factor which produced the results in question in my experiments after the Wundtian method was carried over from the preceding experiments; for, furthermore, the conditions of my last experiments differed from those of Wundt and Geiger essentially in being preceded by these other experiments by a different method.

The factor in question probably consists in the greater attention to the middle point of the scale, resulting from the preceding experiments in which the middle point indicated by the black strip ('goal'), was intentionally made the focus of interest. It is impossible to suppose that the habit of fixing the attention upon the middle position, to the nurture of which the usage of the preceding experiments with these subjects contributed, is not unconsciously carried over into the subsequent experiments, with the effect of influencing the judgments in a manner

similar to that in which the preceding judgments were influenced.

Geiger noted a factor analogous to the one in question, which consisted in the fortuitous effect upon the attention of the uppermost and lowermost positions of the scale. This factor first appeared in the experiments in which the telescope was interposed, thus introducing a division of the dial into quadrants by the cross-threads of the instrument. But as Geiger argues, an influence analogous to this actual partitioning of the dial by means of the telescopic field, is doubtless operative, though of course in a less degree, in experiments of the original form. For there too, the top and the bottom at least of the dial become, as it were, critical points in the path of the index-hand. The very fact of these points being uppermost and lowermost gives to them of itself a feature of distinction. Further, these points represent for us, as they do in all symmetrical objects, median positions. These points possess then of themselves a certain specific interest, analogous to the interest we find in general in the median region of all bilaterally symmetrical objects, as especially exemplified in the human body.

So much for the influence of the 'Umkehrpunkt,' as brought out by Geiger, upon determinations made with the complication-clock. What of its influence upon determinations made in experiments in which the complication-pendulum has been the apparatus employed? From an analysis of the different elements entering into the conditions of experimentation with the pendulum apparatus, we may justly infer that the effect of a critical point is still more potent in the use of this machine, because of the oscillation of the index-hand about a middle point. For in this form of apparatus we have in addition to the effect upon the attention of a middle point of the scale, as cited by Geiger, likewise the influence due to the fact that here the middle point on the scale is also the middle point as regards the path over which the index-hand swings. This latter factor has the effect of emphasizing the middle position in a two-fold manner, that is, in regard to space and in regard to time; for the length of vibration, measured in degrees, of the index-hand

upon one side of the mid-position is exactly equal to the length of its vibration upon the other, and the time of vibration of the index-hand, upon one side of the central point is exactly equal to the time of vibration upon the other. Hence it is the central point, temporally as well as spatially considered. Finally, a further factor tending to give preëminence to this middle position lies in the fact that this position is the only one throughout the entire excursion of the index-hand at which the velocity is constant. Toward this point and only as far as up to this point, the index-hand is increasing in velocity; leaving this point and immediately after passing it the velocity is decreasing, so that independently of the influence upon the error of the factor of acceleration and retardation *per se* (if any such influence exist), this factor tends indirectly to emphasize in a special manner the position at which the index-hand is at its center of oscillation; and thus is added a third element to the general influence serving to emphasize the attraction of the middle or uppermost point upon the scale. Of course this last element, as well as the other elements contributing to the total influence tending to render prominent the critical, turning-point in the path of the index-hand, is throughout unconsciously operative. The question that here suggests itself is whether, after all, the difference in the results on the two sides of the dial, where the pendulum machine was used, was really due to the factor of acceleration and retardation, or rather to this factor of the difference in the influence of the central point in the scale, according as the position of the index-hand was on the one or the other side of it when the bell-stroke or other discrete stimulus occurred.

Naturally the consideration of this factor calls into question the results of the work of Klemm, which, as above stated, was planned with a view to isolating the influence of the factor of eye-movements, according as the judged position was upon the side of ascent of the index-hand or upon the side of descent, from the influence of the factor of acceleration and of retardation occurring concomitantly under the same conditions. Was the factor which Klemm thought he isolated really the factor of acceleration and retardation, or was it the factor which

our experiments have shown to be so considerable under the simpler conditions of experiment obtaining in the use of the complication-clock, and which, it may be justly inferred, would operate so much more effectually with the use of the pendulum apparatus—namely, the factor of the influence upon the attention of the chief or central point in the path of the index-hand?

We have, then, throughout all such experiments, the tendency to elevate in importance the uppermost or, in Klemm's work, the lowermost, region of the dial; but besides this unconscious influence, it is the deliberate intention of my principal experiments to concentrate the interest upon this region, and make of it the goal to be attained by the index-hand at the moment of the discrete impression.

Let us call this factor of the attraction of the predominant point of interest, which is seen to be especially operative in my experiments, factor No. 1. Now, assuming this factor alone to be present, judgments from the 'A' positions, in the normal procedure, in which the subject moves the position of the stimulus directly toward alignment of the index-hand with the goal, would be invariably on the 'A' or positive side,¹ and conversely, judgments from the 'B' positions would be always on the 'B' or negative side. For the effect of the attraction of the objective point or goal is to cause a given position of the index-hand to seem nearer the goal than it really is, and accordingly an actual position, which is a certain distance from the objective point will seem to be at that point. Positions still nearer the goal than that which the subject first judges to be the correct one may also seem correct, as shown in the case of Subject II, who actually moved to such nearer positions and even to a position beyond apparent simultaneity.

Now since results by my original method for each subject in nearly every rate show the determinations, when setting out both from the 'A' side and the 'B' side, to have the same sign, *i. e.*, to be on the same side of the goal, it is evident that

¹ Speaking of course in terms of my experiment, where the judged position was finally zero in each case, and the record of the judgment, the distance of the real position from that judged position.

a second cause or group of causes is operative which is sufficient to overcome the influence of factor No. 1, on at least one side of the goal. Since factor No. 1 changes the direction of its influence when the goal is past, or at least since it becomes a permissive influence in the new direction, the two factors are now really coöperating, *i. e.*, factor No. 1 now probably operates to permit an effect of factor No. 2 greater than it would have if factor No. 1 were not present. Certainly it does not lessen it. For example, Subject I in Table 1, gives throughout (with the single exception of the 'A', 0.75 rate, for light) negative determinations; which means that the determinations both from positions of setting out in the 'A' quadrant and from those of the 'B' quadrant were in the 'B' quadrant. Hence, in obtaining the former determinations the goal must have been crossed, which means that in addition to factor No. 1, which allows the determination to stop before reaching the goal, there was present a further influence which operates to carry the judgment by the goal and over to the 'B' quadrant or negative side.

A glance at Table 1 shows that while the movements from both directions terminate in the same quadrant, they do not terminate even approximately at the same point. From this it is clear that the terminus of the judgment in so far as concerns the influences as yet considered, is not a point but a region within which any point might serve. The subject therefore who proceeds to the position of his final judgment as in the normal way, moves into this zone and stops, whereas the subject who inverts this procedure moves through this zone, and then back into it. We may say then that the difference between the values of 'A' and 'B' represents the width of this indifference zone. What is here called the indifference zone, corresponds to the region to which Klemm applied the name of 'Simultaneitätsbereich.' What the constant factor (No. 2) is, which tends to give the average of 'A' and 'B' a negative or a positive value as the case may be, is a question with which we shall later concern ourselves.

The next factor to be considered is the movement of the position of the discrete stimulus from the starting point towards

the goal. This we may call factor No. 3. Whatever effect this factor may have, we should expect to be dependent upon the length of the movement, *i. e.*, whether starting out from a position at 75° , 60° , or 45° from the goal in one or the other quadrant. It is important to note that since the positions in question correspond on the two sides of the dial, and the movement is in opposite directions on the two sides, this influence must cancel on the averaging together of the 'A' and 'B' results.

Tables 3 and 4 show for the light-stimulus and the sound-stimulus respectively a pronounced effect of the different positions of setting-out on the two sides, the tendency being on the 'A' side to make the judgment more positive, when starting from a more positive position *i. e.*, from a position more remote from the goal; and conversely on the 'B' side, the judgments are more negative according as the position of commencing is more negative, *i. e.*, more remote from the goal. So that, in general, in commencing successively with positions of 75° , 60° and 45° on the 'A' side, the judgments become progressively less positive, while employing successively these same positions of starting out on the 'B' side, the resulting judgments are progressively less negative. This may be interpreted first, as an instance of the prevailing tendency to bring movements and even judgments to an average; or second, it may be regarded as due to an influence of the starting-position on factor No. 1 or on factor No. 2. The exact details of this second interpretation are not apparent while the first is probably true in any event. This influence of the starting-point whether expressed through the averaging of movements or other processes, or through the influence of the starting-point on the attention, and thus on factor No. 1 or No. 2, or on both, contributed largely to the variable error. If each group from a given starting-point is treated separately its variable error is low.

It was stated in the beginning of this section that the deviation in opposite directions of the judgments in the opposite ('A' and 'B') quadrants of the dial, as shown in the experiments I conducted after the manner of Wundt, might be ascribed

to the contrary effects of eye-movement upward in one quadrant and downwards in the other, except that under the conditions of my method of experiment the factor of following eye-movements is practically *nil*. I wish now to show why, in consideration of my altered method of experiment, this hypothesis is not to be entertained. It was the experience of all my subjects that the eyes were kept constantly fixed on the goal. If any eye-movements did occur, they could have been only slight and therefore confined to the middle portion of the scale, in which region the index-hand moved in a nearly lateral direction. Hence, the eyes being fixated upon the central goal with approximate constancy, movements of the eyes in the way of following the index-hand could have been only in a direction practically lateral, not up and down. If, however, we should suppose that larger eye-movements do occur, the effect would be as follows: On the 'A' side, where the index-hand is ascending, the eye-movement, due to the attempt to follow the course of the index-hand, would bring about, as Geiger has shown, a negative tendency in the displacement, and on the 'B' side a positive tendency. Now since a position of the index-hand on the 'A' side always corresponds in my experiments to a positive displacement, and a position on the 'B' side to a negative, the effects of following eye-movements would be simply to diminish the measured error in all cases and not increase the error on one side and decrease it on the other, as in Geiger and Wundt's method. Thus in any case, the results by my 'goal' method do not involve any factor due to eye-movement, except in so far as the separation of the 'A' results and the 'B' results may *possibly* be thereby less than it would otherwise be.

I have referred above (p. 29) to the different orders of succession of the velocities employed, first the order in which the velocities were increased from rate to rate, then the reverse of this procedure and finally the arrangement of the series whereby the extremes of speed became intermediate in position and the intermediate rates occurred at the beginning and at the end of the experimental hour. It might be expected that this succession of rates one after another, particularly in the

last-mentioned order in which the slowest rate is made to succeed directly the fastest, would bring to light some influence of contrast. On careful scrutiny of the results, however, no essential difference appeared between the results for the same rates in the different orders. The method employed tended to eliminate contrast effects, because, determinations being made in groups of six with each rate, and requiring a number of revolutions for each judgment, the contrast effect, if present, would, unless very large, affect only the first judgments at a given rate, and hence would become inconsiderable in the average of the first judgment with the other five. The principal result of a contrast, if present, would be an increase in the mean variation.

Also, because of the order of succession of the four rates employed, the influence of practice does not prejudice the results of my experiments. By practice is here understood the effect of repetition and of adaptation to a constant condition. Since every rate was taken at each experimental hour, the averages given (see Table 1) each represent approximately the same amount of practice. However, the results for each rate were carefully gone over with respect to their time-order, and no variation of the results could be correlated with the progress of the experiment, *i. e.*, with the increasing repetitions.

In my discussion of Geiger's results an account was given of the two methods of observation which this experimenter found to be characteristic of the two types into which he differentiates his subjects, designating them the "naïve" and the 'reflecting' methods accordingly. I stated that Geiger attributed no exact significance to the terms by which he denoted these opposed types, but that he believed the two methods of observation thus designated often merged more or less into one another, so that the distinction which these terms indicated, was by no means a rigid one. Further, Geiger states that the terms are in themselves inadequate to classify the types in question; that, while the subject denoted as 'naïve' has in his general method of determination a tendency more or less aptly described by this term, it is yet unsatisfactory in its import, and he finds the term 'reflecting,' by which he des-

ignates the subject accustomed to the other habit of observation, equally inadequate. It is apparent that the terms are on the whole but faintly descriptive of the subjects' methods, and Geiger is frank to own their ineptitude. He therefore offers as alternative to this terminology the terms 'scale-type' and 'index-hand-type' of judgment—the scale-type corresponding to the 'reflecting' and the index-hand-type of judgment corresponding to the 'naïve.' For, it will be remembered, it is characteristic of the 'reflecting' type that the observer notes quickly and holds fast to the *position on the scale* at which the index-hand arrived, at the moment of entrance of the discrete impression; on the other hand it is characteristic of the subject of the 'naïve' type that he follows the index-hand with more or less of nonchalance until, without effort on his part, its position at the moment of the discrete impression seems established for him, whereupon he notes the corresponding position on the scale.

Undoubtedly Geiger has the right clue in assuming two types of observers, or rather two types of judgment, but his failure to isolate them sharply from one another may mean that he has caught only the one aspect of them which appears in his particular conditions of experiment.

My experiments had not proceeded far, when it became apparent that the method of making determinations was not constant; not that certain subjects followed exclusively a certain method throughout, while others observed according to an entirely different method, but that it was early seen that each subject inclined now to one way of proceeding and now to another way, so that two factors came prominently into consideration according to the methods in question. These factors were in the first place the perception of the *spatial* position of the discrete stimulus in relation to the alignment; *i. e.*, the angle between the goal and the position of the index-hand when the discrete stimulus occurred and secondly the perception of the *temporal* relation between the two stimuli; that is, between the moment of the alignment and the moment of the discrete stimulus, or *vice versa*. We have then two types or methods of procedure according as the spatial or the temporal

factor is the more prominent, and I have accordingly designated them the spatial and the temporal types.

It should be stated that the difference in the nature of the judgment depending upon whether the subject's determination of lack of simultaneity was really the estimate of the purely *temporal* interval between the discrete impression and the alignment, or of the spatial separation of the two corresponding positions of the index-hand, was brought to my attention entirely independently of Geiger's statement with regard to the two types of determination discriminated by him, and it did not occur to me at the time that what I called the spatial and temporal methods of determination could be correlated with the methods he described. That Geiger's 'naïve' and 'reflecting' types correspond to the two forms of judgment revealed in my experiments I hope to make clear in what follows. With Geiger, the 'naïve' type is the type of observer whose custom is to regard primarily the index-hand—to correlate the index-hand *per se* with the discrete stimulus. The observer of this type asks himself *where* was the index-hand at the stroke of the sound; that is, he locates it not with regard primarily to the scale or marks on the dial, but rather by its angular position, probably by reference to the vertical and horizontal orientation. This does not mean that he explicitly recognizes the orientation or measures in degrees. But the spatial position of the hand at a given moment is noticed without the aid of the dial or scale.

On successive rotations of the index-hand, the observer continues to note the positions in the same way, and hence notices the angular distance between the position at which the index appears to be when the discrete stimulus occurs one time, and the position next time. When finally this deviation, which appears to him each time as the *error* in the preceding estimation, ceases to occur, the observer accepts the determination as final, and notes the scale position, if it have not previously obtruded itself upon him. It is seen that in this procedure the subject's estimate of the position of the points of his supposed error and of the consequent correction is expressed in spatial terms. The question of 'how far ahead?' or 'how far

behind?' is the subject's new determination with regard to the preceding apparently correct position is a question of the measure of the angular interval between the positions.

This spatial element in the determination is especially brought out through the method involved in the use of apparatus such as I employed, in which the subject is called upon to perform the active placing of the temporal position of the discrete stimulus at the moment at which the index-hand shall pass a certain definite point on the dial, setting out from various positions more or less remote, upon either side of the position to be attained. It was found that in first setting out certain subjects more readily took account of the spatial than of the temporal interval between the index-position at which the discrete stimulus was given them (75° , 60° , 45° 'A' or 'B') and the position (at 0°) to which they had to shift it. The spatial separation was for them the easier datum to reckon with in this case. Where the interval was wide, as always in the beginning of each experiment, it was the more comfortable way of regarding the separation of the two positions. As the two termini stood out pictorially before him, the subject quite unconcernedly noted them together. As the method involved less the feeling of strain upon the attention than would have been occasioned through the attempt to connect up in a single conceptual act the temporal relation of the two impressions, at least for a certain type of subject, such a subject, upon setting out, fell naturally into this habit of observing. It is because of this characteristic leisure-attitude of the subject in making judgments according to the method which obtains in spatial estimates of the lack of simultaneity that I am led to identify the observer of this type with the one whom Geiger calls 'naïve' or of the 'index-hand' type.

In the second method of observation the time-element is predominant. With this type the influence of the sense of the spatial relation of the two positions: *i. e.*, the position which the index-hand first appears to occupy at the occurrence of the discrete stimulus and the position of zero at which the subject is to place the latter, is apparently eliminated. The subject makes of it a time-judgment, and the experiment becomes

substantially an attempt to reduce an initially large time-interval to zero.

In experiments like Geiger's the 'reflecting' subject watches a scale-division to notice if the appearance of the index-hand at it is simultaneous with the appearance of the complicating stimulus; and if it is not, he tries other scale divisions, to one side or the other of the first. From this description it is obvious that the temporal form of judgment as it appears in my experiments is justly to be correlated with the 'reflecting' or 'scale-division' type of judgment as shown by Geiger. Now the temporal method is quicker than the spatial; that is the subject reaches his final determination after fewer revolutions of the index-hand than are required for the subject whose judgments are of the spatial type. The reason is undoubtedly this: in the temporal form of judgment the subject is dealing successively with two sharply limited, clear-cut impressions; the momentary discrete impression and the momentary impression of the scale-division which the subject selects to note as the index-hand sweeps past it; each is brief and clearly defined. But in the method of spatial determination the subject is necessarily occupied with both details at once, and moreover, perceives the angular position of the index-hand indistinctly on account of its being in motion and traversing the very angle it marks off. Hence the individual judgments are less clear by this method, and more revolutions are needed before the subject arrives at a satisfactory determination, just as Geiger found to be the case with the 'naïve' type as compared with the 'reflecting'. In this agreement in difference of time required for making a judgment, there is then further ground for correlating my temporal and spatial types with the 'reflecting' and 'naïve' types respectively of Geiger.

It must not be understood that the two types which I have designated 'spatial' and 'temporal' are to be ascribed to individuals adhering rigidly to one or other of them: that one observes exclusively with the assistance of spatial data and another type exclusively with respect to temporal data, but rather that they are two methods which coöperate in most cases. It seems probable therefore that each subject is swayed

more or less by the influences of both methods, and that the type can be determined only as the predominance of one of the two methods. As said above, it would seem that in my form of apparatus, the obviously wide spatial interval between the place of the stimulus when setting out and the goal to which it is to be brought renders the spatial method of observation the more convenient at the beginning of a determination. To compute the interval in terms of the spatial perception of its termini is instinctively felt to be the more economical procedure. When, however, the interval becomes narrow (as when the subject has shifted the stimulus toward the goal) and may not be readily computed because of its narrowness the tendency to rely upon the relative *temporal* position of the two stimuli (discrete impression and alignment of index-hand with central goal) doubtless comes more strongly to the fore. And further, precisely in consequence of the obliteration of the appreciable spatial interval—the interval over which the index-hand is required to move—the method becomes *ipso facto* the method of ‘scale-division’ observation, *i. e.*, in my terminology, the temporal method. The data are reduced to two: alignment of the index-hand with the scale-division or the goal, and the discrete stimulus,—flash or sound—which is to be correlated with it.

Of course it is not impossible that the subject of the temporal type should sometimes adhere to this method even in the beginning of an experiment, feeling the temporal relation of the stimulus to the goal when first setting out; but even in the temporal type of subject it is impossible to say how great a part in general the beginning spatial tendency plays in the ultimate determinations.

The two factors of spatial and temporal judgment were introspectively observed by Subjects I and IV, who were convinced (Subject IV was myself) of the validity and importance of the distinction. Experiments were later undertaken with Subjects I and IV, in which the attempt was made to isolate these two factors by having the subject observe at one time the spatial, at another the temporal relation of the stimuli but these experiments were abandoned because of the great

difficulty of adhering consistently throughout to either of the two methods. It was not possible to isolate them adequately, and the devising of methods by which this isolation shall be suitably made belongs to another and lengthy experiment.

It is sufficiently clear, however, that the two factors are present, and my own attempts at an analysis of the elements involved lead me to the conviction that their importance demands a new development of methods and apparatus for adequate experiment in the future upon this problem. Here, I purpose to show that the phenomena of positive and negative displacements depends upon these two types of judgment, and that the heretofore adduced theories are correspondingly inadequate.

Geiger says "The results of the complication experiment appear to contradict normal reason:" for "one would naturally expect that the sound would always be united with a later scale-division than that with which it is actually simultaneous" and the explanation that follows assumes the onus of allaying our disappointment in this expectation. But Geiger neglects one important consideration. Were the assumption true that we are here dealing with a pure time-judgment, then it would undoubtedly contradict the presuppositions of normal experience to record that the subject, instead of selecting a later position than the actual one, should in general, actually select a position on the scale earlier than the real one. But we are not here dealing with a pure time-relation; in other words, the complication experiment has not been, in truth, the problem which all this while it has been tacitly assumed to be, namely, the determination of the *temporal* relation of a discrete impression to a series of similar impressions, into the course of which it enters at regular intervals, but the experiment is seen to become, on analysis, a determination of the *spatial* as well as of the temporal relation of the stimuli in question. Doubtless this prevailing misconception of the essential conditions determining the judgment in complication work is responsible for the wide disparity in the explanations which different observers offer to account for the phenomena, and its recognition reconciles in a measure the fundamental variances amongst them.

Let us come now to the explanation of the displacements, based on the difference in the types of observers, which my method of experiment brings out. In agreement with Geiger and the other German experimenters, I regard the positive errors, *i. e.*, the displacements to a position later than the actual one, to be the more natural errors. On the other hand, in opposition to these theorizers, I hold that these errors would actually be found prevailing were the displacements in reality only time-displacements; *i. e.*, were the measured error an error exclusively in estimation of the temporal relations of the stimuli in question; were the problem simply one of time-relation as psychologists have heretofore implicitly assumed it to be. For even in the experiments which do not exclude the presence of the spatial factor, the positive displacements are the prevailing ones with subjects who are predominantly of the temporal type. For the subject who observes strictly the temporal relation of the stimuli the discrete stimulus is the preëminent one, although it does not necessarily receive the most attention. Besides this, the discrete impression is the more sharply delineated and the exact instant of its entrance is noted with less expenditure to the subject than the momentary position of the index-hand corresponding to it. Hence the discrete impression becomes the determinate term into relation with which the indeterminate impression, namely, the corresponding scale-division must be brought.

The problem then reduces to a question of the just imperceptible time-interval. The subject is to catch the position of the index-hand simultaneous with the discrete stimulus. Adverting from one to the other with the utmost possible promptness, he will normally select a position necessarily somewhat behind, that is, somewhat later than the position to which the sound (or flash) actually corresponds. For, a certain time, however brief, being lost in the transition necessarily, in this time, minute though it be, the index-hand will have traveled over a greater or less interval, and consequently the judgment will fasten upon a position later than the true one. However, Angell and Pierce are wrong in asserting that the subject does not experience the simultaneity of the two impressions, but

that he is satisfied with his judgment when he has succeeded in approximating a position of simultaneity as ascertained by the feeling of a very brief interval between the auditory and the visual impression. The subject, being acquainted with the object of the experiment, necessarily understands that the whole problem turns upon his appreciation of simultaneity, and that he has satisfied the essential conditions of the experiment only when he has succeeded in determining the position on the scale at which the index-hand seems to him to arrive simultaneously with the occurrence of the discrete stimulus. How else could the subject assume the occurrence of the two impressions to be simultaneous than by the subjective consciousness of simultaneity? What criterion has he for determining simultaneity other than the appearance of simultaneity? Now, in a determination made in conformity with the procedure of the temporal type of observer, the place at which the index-hand appears to be simultaneous with the discrete impression will be found to be, as a rule, *after, i. e.*, farther along the scale (in the sense of the direction of the rotation of the index-hand) than the real position of simultaneity, and for the reason that the discrete impression is at the final judgment the primary one, and a certain time-interval between it and the arrival of the index-hand at a certain point on the dial will be unperceived; hence the arrival at this point will be apprehended as simultaneous with the discrete impression.

The procedure just described is normal in determinations in which the prevailing tendency is to the temporal form of judgment; but it is of course probable that the imperceptible time-interval will occasionally be reduced to zero, in which case there will be no displacement; or that the discrete stimulus will sometimes be taken as secondary and the arrival of the index-hand at the point on the dial as primary, in which case a negative error will result. We have here then, an explanation of the occasional so-called correct judgments, though in truth a determination which actually corresponds to the real position of simultaneity is no more correct subjectively than a determination which gives the maximum displacement; for in each case there is subjective consciousness of simultaneity.

Wundt is therefore unwarranted in alleging the presence of these occasional random correct judgments as proof of the ability to experience two impressions as simultaneous, for as an experience of simultaneity it is no more trustworthy than the experience of simultaneity in those cases in which the judgment actually shows a considerable displacement.

As just said, occasional negative displacements are accounted for by the subject's inversion of the usual time-order. But what is the true account of the general prevalence of negative errors, such as shown by all the experimenters up to the present time, with the single exception of Angell and Pierce, whose results showed a prevailing tendency to positive displacements? In my account of the opposed direction of displacements I am led from the results of observation and introspection to rely upon the factor of the opposed method of observation characteristic of the spatial type of observer and I may immediately summon to support my argument the fact just cited, that Angell and Pierce, who throughout their experiments adopted the method of determination recognized by Geiger as belonging to the 'reflecting' type (already identified with the temporal type of my classification), and who were the only investigators to observe exclusively according to this method, were also the only observers who obtained throughout a preponderance of positive displacements; agreeing with the quality of the displacements Geiger found to obtain with those of his subjects who observed according to the corresponding method.

What then was the method adopted by the other investigators? Geiger, who made inquiry of two of those preceding him and even conducted a few experiments upon them in the hope of finding them of the 'naïve' (or spatial) type and so confirming his observation that the 'naïve' observers show negative results, was disappointed, for at least one of the observers (a subject of Pflaum's) proved of the 'reflecting' type. Geiger therefore was led to attribute the discrepancy to the apparatus employed and concluded that the pendulum apparatus rendered difficult the observation according to the strict 'reflecting' method, owing to the difficulty of steadily fixating a single scale-division, due to the variability in the movement of the

index-hand and the resulting obscuration of the scale-divisions in its course.

If the increasing strain of attention described by Wundt is responsible for the occurrence of negative displacements, it would seem to be a more potent factor in determinations of the temporal type; in which the increasing strain of attention may occasion an anticipation of the alignment. But this anticipation, as we have said before, accounts only for occasional negative displacements. But what of the negative displacements occurring in the large majority of cases? To explain these errors which are due, as we have stated, to the spatial form of judgment, it is necessary to invoke the fact insisted upon by Wundt and Geiger, that the interval in the appearance of the two stimuli is a relative, and not, as von Tschisch thought, an absolute one. It is a question of the relative time of apperception, as Wundt terms it, of the auditory and visual stimuli. Now those subjects who identify the discrete stimulus with a position of the index-hand spatially separated from the position on the scale at which index-hand and momentary stimulus are simultaneous, the attention being fixed more upon the discrete stimulus, are led by the sense of its spatial relation to the rotation of the index-hand, that is, by the feeling that the discrete stimulus somehow enters from a position somewhere in the direction from which the index-hand approaches, to identify the discrete stimulus with a position earlier than the actual one in the course of the index-hand. That is to say; to the observer of the spatial type, the traction upon the attention exerted by the feeling of the direction from which the spatially identified discrete stimulus appears to enter—the direction of entrance of the index-hand with which it is allied in space—causes the subject to displace it in the direction of its apparent entrance, thus uniting it with an earlier position of the index-hand than the correct one and giving as a result a negative error. This is the element in determining the judgment which I called factor Number 2, and which has been up to the present undisposed of.

Obviously, the greater the number of repetitions of the stimuli, *i. e.*, the greater the number of preliminary rotations of the

index-hand, the greater the effect of the traction upon the attention by the discrete stimulus due to the influence of the sense of its direction of entrance; the discrete stimulus, as just explained, being identified in the mind of the subject with the direction of movement of the index-hand and having its spatial significance augmented through this association with this movement in space. In this way may be explained the experiments of Geiger in which, observing the error obtaining with each rotation from first to last, the position of apparent simultaneity was found to recede progressively, *i. e.*, to shift with each rotation to a position farther removed in the direction from which the index-hand approaches. This assumption likewise affords an explanation of the tendency to greater negative errors with the slower speeds, as shown by the experiments of the Leipsic investigators; for naturally, the slower the rotation of the index-hand, the more potent upon the attention is the influence of the traction of the discrete stimulus in the direction opposed to that of the rotation of the index-hand, and consequently the wider the displacement in the negative direction.

With subjects requiring a large number of preliminary rotations, the number of such preliminary rotations decreases with practice, hence there is here another factor explaining the greater positivity upon practice, as found by Angell and Pierce and by Geiger. For, the greater the number of revolutions of the index-hand, the more effectual is the influence, as just said, of the sense of traction in the direction from which the complicating stimulus seems to enter; that is, the greater the negativity. With practice the amount of the negative error decreases as the number of rotations necessary to a determination decreases, independently of the change of the subject towards the temporal type. So upon my hypothesis of a space judgment to account for the presence of the negative error is afforded the solution of the problem of the influence of practice in decreasing the negativity of the results.

I have explained Angell and Pierce's positive results as due to their usage of the temporal type of observation. But other influences tending to bring about positive errors in their

results are these: first, they did not take account of the lag in the complicating stimulus due to the interval of time required for the hammer to act and for the transmission of the sound waves from it to the subject's ear; and secondly, the conditions of their experiments were especially favorable to the forward, over-reaching movements of the eyes, caused by the impetus received from the downward course of the index-hand as it enters upon the arc constituting the scale; the latter, as explained before, being placed below the center of revolution and in this regard being unlike the position of the scale with most of the other experimenters. The fact that Angell and Pierce noted no influence of speed is also due to their method of experiment, for speed is of little or no moment in observations of the temporal type, since here it is a question only of the time between a given stimulus and alignment, and the distance traversed by the index-hand in that time is of little consequence. In the spatial judgment, on the other hand, this distance is noted and becomes influential.

The effects of the temporal and spatial methods on the relative distribution of attention to the discrete impression and to the alignment, are yet to be considered. In the temporal form of judgment the attention is maximally on the alignment of the index-hand with the selected point on the dial, for the noting of that is the task to be executed immediately upon the occurrence of the discrete impression. In the spatial or 'naïve' form of judgment the attention is more evenly distributed between the two factors, but is greater if anything to the discrete stimulus, since it is that which picks out a position of the index-hand. Hence it would be expected that voluntary direction of the attention in a way characteristic of the one or the other of these forms of judgment would incline the judgment towards that form, as has been actually found to be the case by Stevens, Titchener and others.

An important point requiring consideration is the hitherto unrecognized question of the possible influence in the experiments earlier conducted of a factor analogous to my 'A' and 'B' directions of setting out and to Klemm's directions of approach from the two sides. We have seen the very striking

effect of the position of setting out upon the resulting judgment, as shown in my experiments (see Tables 3 and 4). Now, the influences of the positions of setting out are here equally balanced on the two sides of the goal and cancel each other in the averages. But what of the influence of the position of setting out upon the results of Wundt, von Tschisch, Pflaum, Angell and Pierce, and Geiger? For, if such an influence were present and not taken into account, operating at one time to displace the judged position in one direction, at another perhaps in the other direction, the consequence of the neglect of so considerable a factor is to render uncertain the significance of the nominal results. Obviously there was such a factor present, for since a determination was not made until after several rotations, particularly in the case of the 'naïve' (spatial) observer, it is clear that corrections were made and there was therefore a point of setting out. Now it is not to be supposed that the subject had always a correction of equal magnitude to make, that he had always to advance or withdraw the position of his determination (depending on whether it appeared to him in front of or behind the new position) by the same amount with every final judgment. Nor is it to be assumed that the position of his tentative first judgment was always upon the same side of the final position in every determination. Yet in the case of some particular subject and rate one or both of these conditions might be realized. Clearly then there was in the earlier investigations the factor corresponding to my 'A' and 'B' factor and experiments conducted without regard to it fail to take account of one of the most potent of the controllable factors occurring in the complication experiment.

To summarize briefly the chief points which my investigation brings out: it shows first the fact that contrary to the implication of Wundt, a displacement occurs where the discrete and the serial stimuli belong to the same sense realm, and even to contiguous portions of the same sense organ, as proved by those experiments in which I employed a momentary light-stimulus instead of the disparate impression employed by preceding experimenters. In the second place, it brings out the difference in the determination depending upon the side ('A'

or 'B'), of the scale from which the subject set out, and the distance of the position of setting out from the terminal position.

And finally, I have shown how the two types of observation found by Geiger, the effects of attention to the discrete impression or to the serial impression, the occurrence of positive displacements predominantly with some subjects and of negative with others, and the positivizing effects of 'practice,' are all brought into agreement and made intelligible by the discovery and analysis of the distinction between the 'spatial' and the 'temporal' forms of judgment.

VITA.

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